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THE PSYCHOLOGICAL REVIEW

AMERICAN PSYCHOLOGY, 1800–1885

BY R. C. DAVIS

Indiana University

Since the great expansion of American psychology in the 1890's, it seems to have been the implicit belief that the 'New Psychology' of the period, largely imported from Germany, was the actual beginning of psychology in this country. While the movement was still young a number of historical retrospects of American psychology were written with but a passing nod to its predecessor as a kind of scientific Dark Age. But now that the brass instruments of the 'New Psychology' have themselves been retired to exhibition cabinets we are in a better position, perhaps, to examine the doctrines of our native ancestors.

In the main, the 'mid-Victorian' period of American psychology may be as dull as it is reputed to be. Yet in the early years of the nineteenth century a more vigorous psychology flourished. The independence and scholarship of the Jeffersonian period continued from the last century. But where there was relatively little concern with psychological matters before, the science began to receive attention in the nineteenth century. The appearance of the first American textbooks of the subject suggests that it was introduced as a special study in the colleges between 1800 and 1830. It was still, of course, regarded as a branch of philosophy, but nevertheless a distinct branch.

The development of psychiatric interest led the well-known Dr. Benjamin Rush to write on psychological topics. His treatise 'Medical Inquiries and Observations Upon the Diseases of the Mind' (40) published in 1812 is the culmination

of his medical-psychological trend, and was probably the first American work on psychiatry. In this he described the cases of insanity he had known, or heard of, from a highly sympathetic point of view. From his many years of practice he was able to describe the development of a deranged condition in patients who seemed to be normal when he first knew them. The 'causes of insanity' he gives as physical and mental, mentioning diseases, poisoning and injury on the one hand, and shock, intense concentration, and worry on the other. His classification of the insanities was very detailed, but it could hardly be called a system, since almost every symptom would constitute a separate class.

Notably included among insane acts were crimes, which Rush believed should be treated with hygienic measures, work, and good handling, rather than by exemplary punishments. Rush's observations on somnambulism were good. He presented certain cases in which somnambulistic activities are forgotten in a normal state, but remembered in a later state of somnambulism. A less happy doctrine, which he felt was somewhat original, was an extreme phrenology. His outline of the genetic development of the mind is at least noteworthy as an attempt.

It would seem to be forcing the author unjustifiably to collect his scattered remarks on psychological principles and try to interpret them as a system. His interest was more medical than philosophical, and the case more important than the rule. Woodbridge Riley (**38**) has classified him as a materialist in philosophy. Certain doctrines to which Rush subscribed would perhaps establish his kinship more closely to that school than any other. The faculties, he says, are to be regarded as simply internal senses. Further, all the operations of the mind are the result of motions excited in the brain. This latter principle is his explanation of dreams as arising from irregular action of the blood vessels of the brain, but is not strictly followed in his explanation that brain disorders arise from mental conditions. Undoubtedly he wrote better as a psychologist than as a philosopher.

Thomas Cooper, younger associate and fellow in misfortune of Joseph Priestley in America, postponed writing on a specially psychological topic until late in life, after he had written on almost everything else and earned the reputation of a troublesome radical. To further his views of strict psychological materialism he translated a work by Broussais 'On Irritation and Insanity' in 1831. As an appendix to the treatise he added a tract of his own 'On the Association of Ideas' (**89**). A large part of this consists in denial—of the existence of a soul, of faculties, of 'common sense.' Mental processes, for him, were well explained as motions in the nervous system. Concerning Hartley's identification of these motions as vibrations, Cooper had his just doubts, and put forward the hypothesis that the galvanic fluid was somehow involved. From the principles of irritability and association he believed a complete psychology could be constructed. This paper, Cooper thought, explained the principles of materialism which were about to blot out the confused teachings of the Scotch realists. Unfortunately for Cooper, the opposite was the case; materialism and sensationalism were practically dead as doctrines in America, even before his book was written.

Intellectually the most remarkable of the early writers was Joseph Buchanan (1785–1829). Living west of the Alleghenies in Richmond, Kentucky, he managed to receive an education at Transylvania University in Kentucky, to study medicine in Philadelphia, to return to Richmond and engage in the practice of medicine, to assist in the attempted founding of a medical school at Transylvania, to introduce a course of lectures there on human nature, and later publish them as a book, 'The Philosophy of Human Nature,' in 1812 (**6**). In his spare time he is said to have invented a vehicle which would run by steam, a 'color organ,' and a new means of developing power, and at one time or another he edited four periodicals. But what seems to be the best American work in psychology of the time received little more attention then than now.

Buchanan was evidently thoroughly familiar with the works of the English philosophers of the eighteenth century. He

mentioned as his chief sources Locke, Hume, Hartley, Erasmus and Robert Darwin. He shows acquaintance with the doctrines of the Scotch school, though he did not quote them. Stemming from the skeptical and materialistic English, he nevertheless advanced the doctrines beyond his authorities, justifying his own remark that his work contained original material.

His position concerning the body-mind problem he formally stated as a double-aspect view—certainly quite unusual for the time. "If it be proven," he wrote, "as it now seems to be, that the intellectual transaction of feeling, thought, volition, made known to us by consciousness, correspond as their shadows to the physical transactions of the brain discovered to us by our senses; may we not conclude, that they are identically the same things perceived by us in different ways and therefore thought different?" To be sure he lays greater emphasis upon the 'physical transactions of the brain' which are—or might be—'discovered to us by our senses.'

Buchanan had a tendency in the direction of psychological experimentation. To illustrate the rapidity of the association process he described post-rotational nystagmus, and the after-image of motion which he obtained by fixating on a rapidly flowing stream. He reported receiving a permanent injury to one eye from observing after-images of the sun. He described as follows his experiment of observing the sky through a window lattice, and then fixating on a white wall: "The latter parts of the eye—those receiving the impression of the lattice—must retain their excitability in a more perfect state; and when an equal impression on all the retina is afterwards made from the white wall, they must hence act with more energy than the others, producing on a light ground corresponding to the former wall, the delineations of a bright lattice work with darker lights."

The phenomena of vision were particularly interesting to Buchanan. He followed in his account the work of the little known Robert Darwin,[1] whose paper on vision was published

[1] Robert Waring Darwin, son of Erasmus, father of Charles, was more than a rule-of-thumb country doctor. The Dictionary of National Biography describes him as not being interested in scientific generalization. (Art. Charles Darwin.) His paper

in the Philosophical Transactions of the Royal Society of London (**10**). A set of laws of color mixtures, essentially correct, was given along with an explanation of the persistence of vision after the object was removed.

In his concepts of neural action Buchanan was peculiarly fortunate for his time. Instead of using the then common theory of 'neural vibrations' he hit upon the analogy of a train of gunpowder as best describing neural propagation. For some reason he was convinced, also, that for a short time immediately after a nerve had been excited it lost its excitability.

Buchanan gave a strictly associational account of will and emotion. In the latter subject he notably remarked on the predominance of sensations from the viscera, particularly the stomach. These suggest to him the possibility of an internal sense, in addition to the classical five departments.

The temper of the times, the geographical origin of his work, or some other factors, made Buchanan's book as obscure as his other enterprises. Woodbridge Riley (**38**) has found mention of the work in a letter Jefferson wrote to John Adams, sending him a copy of the book. Adams failed to appreciate the book and we are not told how it had been received at Monticello.

In 1819 in Philadelphia a work by Ezra Stiles Ely, D.D. (**11**), foreshadowed the predominance of the 'Common Sense' psychology in the middle of the century. The conversations therein set forth between professor and pupil were little more than a guide to the work of Reid with a set of footnotes drawn almost entirely from that author. In contrast to later works this one might be described as Scottish in doctrine, rather than Scotch-American. Like them, however, it is highly didactic and with little evidence of scholarship.

A more original work, on the other hand, was Frederick Beasley's 'A Search of Truth in the Science of the Human

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on 'ocular spectra,' meaning after-images, reports experimental observation of practically all the important phenomena of after-images and the effects of background and projection field upon them. He developed the theory of the after-image as caused by increased or decreased excitability of a certain area following upon stimulation, and a theory of correlations determining positive and negative after-images. Robert Darwin was elected Fellow of the Royal Society.

Mind' (5), published three years later. Beasley was educated in Princeton, and at the time of his writing held office as Provost of the University of Pennsylvania. While in Princeton, he had become acquainted with the idealism of Berkeley, and with the Scotch realism taught in the university at the time. Somewhat unkindly, perhaps, Woodbridge Riley had classified him with the school of Princeton realists against whom he was in revolt. Doubtless he should be called a realist in philosophy, but his psychology at almost every point differs from that of Reid, following instead the footsteps of the author's venerated John Locke. Both English and Scotch writers he thought had been misled regarding Locke's doctrines of ideas; Locke he thought really held a theory of direct perception. The psychology which Beasley developed was emphatically dualistic, anything dubbed 'materialism' being assumed to be incorrect. But far less is left to the 'action of the mind,' than by Reid and his followers. "The action of outward objects upon the senses, and the organs of sense upon the mind, is the real and efficient cause of perception," he wrote, attacking the occasionalism of Reid's 'mental activity' doctrine of perception. A 'passive' theory of memory was also given, and Beasley very wisely avoided the Scottish doctrine of rapid, unconscious but voluntary acts of mind during remembering. He adopted the term 'intuition' so characteristic of the Scottish school, but argued that an intuition was a judgment posterior to experience and based on particular experiences, thereby maintaining adherence to the Lockean dogma of "nothing in the mind except what is derived from sensation and reflection." Possibly an acquaintance with Rush or his work is indicated by Beasley's section on delirium, dreams, sleep, hallucinations and similar phenomena.

This defense and interpretation of Locke was evidently little known. There are few if any references to it in later works, and today the volume is relatively rare.

A period of American psychology from 1827 to 1845 was dominated by the work of Thomas Upham. Psychology as a subject of instruction was apparently quite common: Up-

ham's book (**49**) went through many revisions and editions, and current with it were a number of other works of similar character. Upham began his literary work in 1819 when at the age of twenty he published a book of verse called 'American Sketches' (**48**), which were intended to awaken people to the literary possibilities of the new land. Eight years later, as professor of mental philosophy and instructor in Hebrew at Bowdoin College, he published his 'Intellectual Philosophy' (in an early revision called 'Mental Philosophy'), which in various elaborations was long standard and much quoted. In 1840 he won the prize in a competition of the American Peace Society with an essay on a congress of nations (**51**). He closed his productive career in 1872 with a collection of hymns entitled 'Christ in the Soul' (**53**).

The 'Mental Philosophy' is large, pretentious, didactic, rather uncritical, somewhat moralistic, and moderately religious. In the introduction to an early edition (1828) the author wrote, "My sole aim was the good of young men. . . . In selecting facts in confirmation of the principles laid down I have sought those which not only had relation to the point in hand but which promised a degree of interest for young minds." A robust interest in everything and anything is really the saving grace of the work.

The theoretical position of the book is nondescript. Sometimes Upham wrote as a follower of Locke, regarding ideas and association, but there was also a section on the Scotch doctrine of intuition. Incompleteness and redundancy in system did not concern Upham greatly. His book was heavily over-weighted in its treatment of reasoning, about 35 per cent of the whole being devoted to that topic. But a variety of interest rather characteristic of the period also appears in Upham's work—sections being given to hypnosis, suggestion, dreams, abnormalities, language and æsthetics.

At the end of the 'Mental Philosophy' a large section—10 per cent of the book—is devoted to the relations of psychology and religion. The tenor of this essay is that the principles of mental philosophy will provide a sure foundation for true religion. This interest seems to have absorbed Up-

ham more and more. In his 1840 edition he wrote of psychology ". . . while it might commend itself with some degree of confidence to the philosopher, should, at the same time, be according, as Philosophy ever will be, with the principles and interest of correct morals and religion."

Aside from various expansions and condensations of the 'Mental Philosophy,' Upham published 'The Will' in 1834 (**50**) and a book on abnormal phenomena in 1840 (**52**).

Courses in psychology during this period were evidently less highly regarded by students than by the faculties. In 1829 the American editor of an English work produced the book in hope of making the subject more interesting, "as the science is taught with little success even in the highest schools at the present." In 1838 Upham condensed his 'Mental Philosophy' to about 500 pages for use in high schools and academies. One young lady student inscribed at the end of her copy the following quotation from Dryden's 'Virgil':

. . . No more she said
But in her azure mantel wrapped her head,
Then plunged into the stream, with deep despair
And her last sobs came bubbling up in air.

Other books of the period for the most part followed the pattern of Upham, with greater emphasis on one or another aspect. Sawyer (**44**) and Mahan (**27**) presented the same unorganized mixture of Locke and Reid as found in Upham. An anonymous book called 'Mental Guide' (**1**) has more of Locke, followed by applications to methods of study and to rhetoric. The work of George Payne (**31**), apparently published first in England, foreshadowed the great cloud of Scotch psychology which was to appear in the next period. It received the following endorsement from a professor in Princeton, stronghold of the Scotch philosophy: "There is, I believe, not a sentence in the volume which has an unfavorable bearing on the soundest principles of religion and morality." The religious aspect was still more emphasized in a book written by Mrs. Elizabeth Ricord (**37**) for use in a young ladies' seminary. The introduction informs that this is a

Psychology 'Based on the Truth as Revealed in the Sacred Scripture.' The work is interspersed with quotations from the religious poets and the style is incredibly sentimental.

In this period, but somewhat outside its intellectual trend was Frederich Rauch, whose 'Psychology, a View of the Human Soul, Including Anthropology' (34) was published in 1840, just before he died at the age of thirty-five. Rauch was a native of Germany and was educated in Marburg, Giessen and Heidelberg. In Heidelberg it was reported he was the favorite pupil of the Hegelian philosopher Daub, and after a short term as professor extraordinarius in Giessen, he was appointed regular professor in Heidelberg. But his academic career there was cut short because of some unorthodox remarks on government made in his lectures, and he was forced to leave Germany in 1831. After holding several minor positions in America, he served as president of Marshall College from 1836 until the time of his death.

His work in psychology might be described as a modification of Hegel though little occupied with Hegelian dialectic. Quoting chiefly from Kant, Hegel, Herbart, Carus, Daub and Rosenkranz, Rauch nevertheless described his book as the first synthesis of German and American psychology. Expressing dissatisfaction with current views of body and mind as two substances, Rauch defended the conception that the soul is a kind of vital nexus, that which makes the body what it is. Soul and body, he wrote, are related to each other as the sunlight and raindrops in the rainbow. Independent faculties were denied. Much emphasis was laid on the development of mind as an unfolding, analogous to the growth of a plant from a seed. The psychology proper was in two divisions, on the reason and on the will. Reason consisted of sensation, conception, and beyond these, pure thinking. The will, in the sense of a natural phenomenon, was said to be strictly determined. The first part of the work, entitled anthropology, is a treatment of the influence of the environment on mind, and of the mind on the body and environment. In this, the most readable part of the book, are discussions of racial, sex and individual differences, the effects of environ-

ment upon them, of sleep, dreams and temperament. A final section of the book was concerned with religion and expresses the author's preference for a "religion of understanding and cool reflection."

A contemporary American reviewer (James Murdock, 1842) (**4**) estimated that "Dr. Rauch was one of the class of German philosophers, who, embracing fully the transcendental speculations of Schelling and Hegel, have labored to reconcile them with the religion of the bible." He complained, however, that Rauch had not gone far enough in the direction of true religion, in that he made no place for revelation, apostasy, savior, forgiveness, atonement, judgment or retribution. In other words it would seem necessary to admit that Dr. Rauch was definitely unorthodox.

There was another German writer in America at the time: S. S. Schmucker, professor of theology at Gettysburg. His work 'Psychology, or Elements of a New System of Mental Philosophy,' 1842 (**45**), he believed to be derived entirely from a ten-year study of his own mind, a mind which appears, however, to have been well-furnished by the Hegelians some time before. In comparison with Rauch's, Schmucker's book is very dull reading.

Through the years 1845 to 1885 the psychology written and taught in America was unquestionably poorer than that of the half century before. The psychological principles of the Scotch philosophy were not only dominant but practically universal. Repeated again and again one finds the doctrines of mind as an active entity, of the faculties as modes of activity, of the intuitions of the mind and of the freedom of the will. Woodbridge Riley speaks of the period as the glacial age of American philosophy, in which the frigid wave of Scotch doctrine moved irresistibly down the American valleys. The paralysis was similar in psychology.

Yet it is hardly just to characterize the movement as Scotch philosophy; rather the works appearing were successive dilutions of Reid, Brown, and later of Hamilton in the interests of religious orthodoxy. Thus the mid-century tradition was a kind of Protestant scholasticism, deriving psychology

from theological dogma. A frank example of the procedure is to be found in E. J. Hamilton's (17) argument for the independence of the soul written at the close of the period 1886: "To us, assuredly, those works of wisdom and power and goodness which alone enoble the universe and make it glorious manifest a Being inconceivably great and mighty, yet possessed of attributes essentially similar to those which characterize our own spirits. But where is the brain that gave birth to the omnipotent, all creative mind? Is not the intelligent activity of the creator a case in which the attributes of the spirit are exercised without any connection with the cerebral organs?" This is headed "God has no Brain."

During most of the period scholarship was at a very low ebb. Mark Hopkins' remark, as quoted by Stanley Hall (15), that he had never read Kant because he found him too difficult, appears to have characterized the attitude of most of the writers. Contact with European thought—English, German or French—was practically broken. Few references to anyone are made in most of the books, though lists of half a dozen writers are sometimes mentioned in the prefaces as authorities.

The range of interests during this period was highly restricted. Sections on abnormal phenomenon, animal psychology and social psychology, such as are found in the earlier periods, are entirely missing in the standard texts. Physiological psychology was omitted altogether. The intellectual processes assume such importance that often the author will treat no other subject.

The purposes of the writers were beyond reproach. They wished to give the students that which would be useful in their lives; accordingly the most important contribution psychology could make, the authors thought, was a sound foundation for morals and religion. Of course, the authors' own interests led them in the same direction since theology and morals were so important at that time.

The results for psychology were sometimes disastrous; as in 'Physiology of the Soul and Instinct as Distinguished from Materialism with Supplementary Demonstrations of the

Divine Communications of the Narrative of the Creation and the Flood,' by Martyne Paine (1872), distinguished physician and co-founder of the medical school of New York University (**30**), and 'Autology—A Vindication of the Manhood of Man, the Godhood of God and the Divine Authorship of Nature' (**16**), 1872.

During this period of the academic predominance of Upham, there appeared in America two popular movements of pseudo-scientific nature. Publications on phrenology and mesmerism began to appear in the 1820 decade and soon dotted the landscape from Vermont to Kentucky. Interest in phrenology reached a high point in the eighteen-thirties and forties, but continued to produce large numbers of works until the end of the century. Works on mesmerism were not concentrated in any decade, but were numerous in the middle of the century. The writer has a bibliography compiled from the Library of Congress, listing, between 1820 and 1885, 72 works on phrenology and 51 on mesmerism, under various names. It would be an extraordinarily faithful historian who would attempt to survey this material in detail.

Almost all of the textbook writers of the period were college presidents, who also taught philosophy and psychology. The course in psychology or mental philosophy seems to have been given in the Senior year and, like all courses of the time, was required for all students. The early aversion for the subject on the part of the students was still current, one may judge from the remark of Lyman W. Hall (**14**), 1850, who, of course, hoped to remove their dislike with his new text. Methods of instruction evidently continued unchanged. At the bottom of each page he inserted questions, generally trivial ones, which were to be answered by literal quotations from the text. Noah Porter (**33**), 1878, defended the recitation method of college instruction with heavy arguments, maintaining that the new-fangled lecture method should never replace the daily quizzing and grading on certain assigned pages in a text. Accordingly his 'Human Intellect' (**32**), 1868, used three sizes of type, and material in the large size was supposed to be committed practically verbatim.

In the early part of the period favorite texts were those of Wayland (**54**), 1852, and Haven (**19**), 1857 (judging from the number of editions issued). Wayland, a president of Brown University, was known for his brusque but kindly personality. His prestige and literary style probably accounts for the popularity of his book. Directness of expression and orthodox views no doubt made Haven's book satisfactory. Similar books abounded; those by Champlin (**7**), 1860, Taylor (**47**), 1858, Winslow (**55**), 1850, Alden (**2**), 1866, and Hopkins (**21**), 1873, from our point of view seem to differ from one another only in small details. Most, for example, follow Reid's doctrine of direct perception; a few follow Hamilton's theory of perception as mediated by neural and physical processes.

In the latter part of the period a few books appeared which are individually noteworthy. Noah Porter and James McCosh,[2] pillars of conservatism in education, presidents respectively of Yale and Princeton, seem to have deserved their reputation as intellectual giants of the time. Both were scholars of ample, or more than ample, learning. Porter's 'Human Intellect' (**32**), 1868, later reduced for use as a text, was written much on the plan of Sir William Hamilton's 'Lectures on Metaphysics,' with historical summaries and citations on all possible points. Whereas Hamilton quoted mainly the classics and scholastics, Porter paid most attention to eighteenth and nineteenth century writers, seldom agreeing with them unless they were of the Scotch school. Arguments were minute to the last degree. In doctrine Porter's starting point seems to have been mostly Hamilton's, but he was sufficiently aware of contemporary experimental work to mention J. Müller, Fechner, Wundt and Weber, among other

[2] President Porter's book on higher education was chiefly a defense of the old American college system against innovations such as were being adopted at Cornell. President McCosh debated in New York (1885) with President Eliot on the 'New Departure in College Education' (**25**), 1885, his remarks being better described as an attack on the new system. In peroration he pleaded "Some timid people will say, 'Tell it not in lands whence our pious fathers came that the college whose motto is *Pro Christo et Ecclesia* teaches no religion to its pupils. Tell it not in Berlin or Oxford that the once most illustrious university in America no longer requires its graduates to know the most perfect language, the grandest literature. . . . But whatever others may do, *I say*, *I say* let Europe know in all its universities. . . .' "

German writers. His historical summaries seem to be correct and thorough and still intriniscally valuable.

James McCosh came to America in 1868 as president of Princeton, after a notable if not distinguished academic career in Scotland and Ireland. In coming, he wrote, it was his mission to bring to America the sound doctrines of Scottish philosophy, thereby saving this country "from the errors of Idealism on the one hand and Agnosticism on the other." He surely must have been aware that American writers had already heard of the Scotch; in any event his mission failed, for instead of being the first of the American intuitionists, he was the last of any distinction. In fact, of his psychological writings, only the treatise 'The Emotions' (**22**) and 'Intuitions of the Mind' (**23**)—if that be psychological—fall within this period here surveyed.

McCosh was Scottish in his vigor and independence as well as in his philosophy. 'The Emotions' displays original treatment of a topic almost neglected by the preceding Americans. McCosh's fundamental theory is that emotions contain four elements: appetences, primary and secondary; ideas; excitement with attachment and repugnance; and organic affection. His treatment of these elements is followed by a more conventional classification of emotions according to their past, present or future reference. A final section of the work is devoted to the part played by emotions in society, particularly with an attack on the hedonism assumed in political economy.

Recently Ruckmick (**39**) has observed a foreshadowing of the James-Lange theory in McCosh's statement that deliberately produced bodily states will induce a corresponding emotion. For McCosh, however, this was a secondary, associational phenomenon. Concerning the organic affections he wrote almost as though he were aware of the James-Lange theory and were refuting it. "The mental emotions are not the effect; they are rather the cause of the bodily movements. Some physiologists write as if emotions were a sort of reflex act. . . . In all cases the emotion begins within, in an appetence or affection of some kind, and in the idea of something to favor or thwart."

The 'Intuitions of the Mind' contains little that would be considered psychology today. It is a rigorous defense of one key point of Scotch philosophy against German idealism. McCosh's general work, 'Psychology, The Cognitive Powers' (**24**), 1886, and 'The Motive Powers' (**25**), 1887, appeared at the beginning of a new period in psychology and shows the influence of the new trends by including, for example, diagrams and descriptions of sense organs and brain. As the sub-titles suggest this new material was fitted into the old framework.

The work of John Bascom (1869), professor at Amherst, later president of the University of Wisconsin, is intended to place the intuitive philosophy on firmer ground, but is distinctly better than most avowing such an aim. The author spoke of the work of Johannes Müller, and gave a great deal of attention to Bain, Mill and other English writers. A chapter in the general text is devoted to the nervous system and his work in comparative psychology (**4**), 1878, a reinterpretation of the empiricists, bespeaks an unusual interest. Bascom believed he had reconciled realism, idealism and materialism by regarding them as the results of the operations of three different faculties of the mind. Stanley Hall (**15**) referred to Bascom, whose student he was, as stimulating his first interest in psychology.

Probably it is the system of psychology perpetuated in the academic orthodoxy of the mid-century which is now referred to as the 'old faculty psychology.' A better term would seem to be 'mental activity psychology.' The term 'faculty psychology' seems to refer to a system based on a collection of mental agents such as memory, reason and the like, perhaps phrenologically located. In most of the writings of the Scotch-American traditions are to be found condemnations of such a system at least as vigorous as those written today. Sir William Hamilton (**18**) quoted, in one of his impressive lists, attacks on such a faculty doctrine by twenty-six authors, from Galen to Philoponus among the ancients and from Scaliger to Kant among the moderns, and American writers in general made similar statements. It is to be wondered

whether such a faculty psychology ever flourished at all outside the minds of its deadly opponents. 'Faculty,' of course, was a favorite term of the nineteenth century Americans; they were careful to say, however, that they meant by it merely a mode or manner in which the mind acts, not a separate substance or causal agent. Neither were faculties regarded as constant endowments of an individual; rather they were supposed to be rudimentary in mental deficients, and capable of development in children.

Another characteristic doctrine of the period, that of intuitions of the mind, originally was described as intuitive judgments by Reid (36). Hamilton, speaking of them as the regulative faculty, regarded them rather as modes or categories of experience. Most Americans followed Reid in taking intuition to be a kind of mental act. Reid outlined five criteria for deciding what principles are intuitively appreciated, the most important criteria being general acceptance and utility in avoiding absurd actions. Under his rules Reid listed ten intuited truths which might be summarized as stating the real existence of perceived objects and of consciousness, free will and the homogeneity of nature. In his edition of Reid, Hamilton thought it necessary to construct a retaining wall about these intuitions by limiting exercise of commonsense judgment to philosophers. But Reid had specifically admitted the judgment 'of the ages and nations, of the learned and the unlearned,' and it would seem then that anyone would be privileged to extend the list if he could show that other ideas are widely held. In Haven, therefore, the list of intuitions was space, time, indentity, causality, beauty and moral rectitude. Porter reached a rather high point by listing existence, diversity, identity, time, space, causality, substance, matter and design. The last, of course, is not the familiar argument from observed design but an assertion that design is a necessary assumption before observation could begin. In this manner Porter intentionally hypostatized a whole system of theology.

The third characteristic doctrine of the school, that of freedom of the will, was usually treated by an involved dis-

cussion of the nature of causality in which it was maintained that the object or event was merely the occasion, rather than the cause of the mind's acting. The mind is therefore its own efficient cause for action and the will is free.

It has been remarked that the Scotch philosophy was favorable to psychology because it was founded on psychological theory and encouraged the observation of mental phenomena. Logically perhaps it should have, but there is little indication that it actually did. Even in the best of the works the evidence is nearly always of an inferential or anecdotal nature.

Why this 'intuitional mental philosophy' in the course of a few years supplanted vigorous earlier movements, engulfed the country, and, with rather stultifying effect, remained the standard in practically every college for nearly half a century is an historical problem without a complete answer. It is true that contact with European thought was lacking, but there was little to prevent such contact as later developed if the Americans had desired it. Woodbridge Riley suggested three factors as responsible for the 'glacial period': the Scotch immigration to America, the dominance of Princeton Theological Seminary and Princeton College, long a seat of Scotch realism, and the temper of the times. There was of course a large Scotch immigration in the early years of the century, but during the period an important number of Germans, some of high intellectual ability, also settled in America. It is noticeable also that the orthodox psychology was particularly strong in New England, largely inhabited by the descendants of the original English. Princeton was, to be sure, dominant in Presbyterianism, but no textbook or treatise on psychology except those of the belated McCosh seems to have been written in that college. Further, a survey of the biographies of the authors of the period brings to light no mention of a period of study or teaching at Princeton by any of them. Most were born in New England and New York, and educated in the colleges of those states. Evidently the Princeton influence, so far as it operated, was indirect. The main factor in dominance of the 'mental activity school,'

then, seems to be its conformity with the temper of the times. It coincides roughly in origin with the spread of evangelical religion. In an age so engrossed in theology and morals it conformed readily to cherished dogmas of independence of soul, free will, conscience, value and divinity. The doctrine of intuitive truth made the system especially attractive. Theologians could rejoice in the fitting of their teachings upon a psychological foundation, whereas in fact the psychological foundation merely reflected the metaphysical superstructures.

Other philosophical movements, of course, were present, notably transcendentalism and St. Louis Hegelianism, but neither had great influence upon psychology, perhaps because psychology was so largely in the hands of the presidents of the colleges, often denominational, who were likely to be elderly men, with clerical connections and strongly conservative.

There were a few writers of the mid-century outside the tradition, some even in rebellion. Laurens P. Hickok (**20**), 1854, teacher of Bascom, was original for his time in that he wrote somewhat in the framework and terminology of Kant, arriving, however, at much the same position as the more conventional authors of the period. Evidently he knew something of German authors, but he mentioned no authorities, other than the Bible, for any of his statements. T. Wharton Collins, geographically removed from the main current of thought, was professor of political philosophy in the University of Louisiana. His book 'Humanics' (**8**), 1860, was based largely upon Aristotle and dealt with man as a social individual. Oliver S. Munsell (**28**), 1871, president of Illinois Wesleyan College, possibly was under the influence of the St. Louis group and wrote a text making use of the Hegelian system. He still kept, however, the theory of intuitive knowledge.

A strongly opinionated Kentucky physician, C. Graham, produced two works (**12**), 1869, (**13**), 1859, stoutly attacking the contemporary teachings of free will and moral intuitions. Believers in free will, he maintained, were intellectually dishonest, and conscience, for him, was obviously a mere sum of common ideas. The books have a rather Lockean background

but they have very little to say on other points than these two, which seem to have troubled the author greatly.

A contribution of real originality was that of W. D. Wilson, of Cornell, whose title 'Lectures on the Psychology of Thought and Action, Comparative and Human' (**56**), 1871, has a distinctly modern sound. He 'proved the existence of the mind' but admitted reducing its importance in thought and action. 'Freedom of the will' was maintained but on introspective rather than *a priori* grounds. The works of Maudsley and Carpenter were quoted and it was in this book that description and diagrams of the nervous system first reappeared in a psychological text. Direct perception of objects, as insisted upon by most realists, was explicitly denied. The style is simple and clear, and, particularly in the section on will, the work might almost be taken for an early production of William James.

Although he was largely interested in logic and mathematics, two other psychological works came from Wilson's pen, one on the relation of language and thought (**58**), 1875, another on psychology and metaphysics (**57**), 1877.

Lewis Henry Morgan, father of American anthropology, produced in 1868 a work on the American beaver (**28**), which his biographer, Stern (**47**), believes should constitute him also the founder of animal psychology. It is a report of a first hand study of beavers in their natural habitat. His interest in the subject and his psychological interpretation that animals behave in accord with the same principle as humans, *i.e.*, mind, were certainly advanced for the time. The chief superiority of humans over animals, Morgan maintained, is the human possibility of transmission through language. The human psychology which he extended to animals was, however, the usual 'mental activity,' 'faculty' doctrine of the time.

Out of the evangelical revival and the popular excitement concerning mesmerism in the middle of the century arose the strange figure of La Roy Sunderland. With little formal education, he was converted to Methodism at an early age and soon began to preach to Methodist congregations. During

his first sermon a dozen of the congregation fell into a trance state as he spoke and after this impressive beginning he was in great demand for revival meetings. After ten years he retired from the ministry and devoted himself to writing and lecturing. He had become interested in the causes of the phenomena which he had observed in his audiences and concluded that it was a state of hypnotism rather than a 'visitation of the Lord.' For the next forty years, therefore, he attacked the religion he had previously preached. His theory of 'pathetism,' as presented in the 'Book of Human Nature' (46), 1853, supposed to explain the trance state, was fundamentally the idea that one mind could influence another through expressive gestures. To induce a trance, the operator should picture in his own mind the state desired and execute appropriate gestures. Sunderland's writing was a hodgepodge of disconnected paragraphs, sometimes exhibiting a keen insight, sometimes a perfect gullibility.

As the account of American psychology of this period began with Benjamin Rush, it may well be closed with a mention of his son, James Rush. He graduated from Princeton and received a medical education in Philadelphia and Edinburgh in the early years of the century. He practiced medicine but in a few years he retired to devote himself to study and writing of both literary and scientific nature. He was able to shift from a scientific to a literary mode of thought, he wrote, by elevating his feet to the table and sitting on his spine in 'the Nassau Hall position.' In this mode he produced 'Hamlet, a Dramatic Prelude' (41), 1834, in blank verse. His scientific works are 'A Philosophy of the Human Voice' (42), 1827, which is chiefly a work on rhetoric and oratory, and a two volume work with the prodigious title 'A Brief Outline of an Analysis of the Human Intellect Intended to Rectify the Scholastic and Vulgar Perversions of the Natural Purpose and Method of Thinking by Rejecting Altogether the Theoretic Confusion, the Unmeaning Arrangement and Indefinite Nomenclature of the Metaphysicians' (43), 1868. Rush indicated in his preface that he had in his younger days read considerably in the field of psychology, but

finding all the works 'metaphysical' and worthless, he had forgotten them and set out to write a scientific treatise. The two writers whom he quotes with most regard are Bacon and Descartes, both for their works on scientific method.

His position on the mind-body problem was similar to that of the English cerebralists, though undoubtedly arrived at independently. The ultimate method of studying psychology, he thought, would be to apply some instrument to the brain which would reveal its actions. (He thought that the microscope might at some future time be adapted to the purpose.) Lacking direct information he was forced to write in terms of sensations and ideas which were to be understood as imperfect approximations to brain processes. The first volume of the work is largely a catalogue of mental experience, classified according to its object. Throughout the work, on the slightest provocation, he belayed the metaphysicians with a patrician fervor. Volume Two was almost wholly occupied with a series of sardonic 'characters' after the manner of La Bruyère. Here were portrayed for example the character of man, of woman, of the politician, the lawyer, the physician, the teacher and the republican voter. Bacon and Shakespeare are the only ones to come through unscathed. The portraits still make excellent reading, provided one assumes the 'Nassau Hall position' for their perusal.

Rush's work, to judge from his own statement, went without recognition in his own time. "Nearly forty years ago," he wrote in the preface to the 'Human Intellect,' "The Author gave the majestical pretenders to intelligence fifty years to comprehend the first part of his work, 'The Human Voice.' He finds he mistook their capacity. On the Second Part he will be more liberal . . . he here allows them three hundred years in which to clear away their piles of rubbish and try to reconcile themselves jointly both to the first part and to it."

Upon his death, in 1869, Rush left a fortune of one million dollars to the Philadelphia Public Library. At present that library does not possess a copy of his chief work.

The recession of the Scottish mental activity school of psychology in the closing years of the century is even more

difficult to account for than its earlier rise and predominance. There was not, of course, a return to the interests and principles of an earlier period, but rather an importation of new viewpoints, such as romantic idealism, Herbartianism and experimentalism from Germany, and an original American development in James. There was contact with Germany, of course, but this no more explains the phenomenon than the absence of contact explained its rise in the earlier period. It would likewise be a matter of great interest to trace the influence of the mental activity psychology upon the men of the early 'Scientific' period. Their education and early association with that school would very probably be found to have had a molding influence upon contemporary, American psychology.

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THE NATURE AND PHYSIOLOGICAL BASIS OF VISUAL MOVEMENT DISCRIMINATION IN ANIMALS[1]

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Introduction

The important rôle of moving stimuli in the determination of responses of lower animals has been recognized by many close observers of animal behavior. Washburn (**68**) explains the necessity of responding to moving stimuli by referring to the evolutionary significance or adaptive nature of such response. ". . . a moving stimulus, one acting successively on neighboring points of a sensitive surface, produces an effect disproportionate to its intensity. A moving stimulus is a vitally important stimulus: it means life and hence may mean either food or danger." Koffka (**32**) also points out that many of the lower animals will react only to movement of light patterns.

Such observations as these, combined with the continued interest in the stimulus characteristics of visual movement in the study of human perception, have encouraged the writer to attempt to summarize the scant but increasing experimental data on visual movement perception in the phylogenetic series. Koffka (**32**) and Hovland (**27**) have recently reviewed the available evidence on movement perception, both real and apparent, in normal human subjects. Some of the results of experiments on humans will be included in the present discussion for purposes of comparison in relative acuity.

Furthermore, it is suggested that an adequate physiological theory of visual movement perception can not be reached without taking into consideration: (1) the general anatomical

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findings on the structure of the visual system; (2) the results of physiological studies on movement discrimination in animals with different parts of the nervous system removed; (3) the results of clinical investigations on human subjects with lesions in the occipital lobes. These three considerations will be covered in some detail since they provide the fundamental basis upon which a physiological theory of movement perception may be erected most profitably.

I. The Nature of Visually Determined Responses Based on Moving Stimuli

A. The Stimulus for Visual Movement Discrimination.—The light energy which serves as the stimulus in studies of the mechanism of visual reception may, for purposes of convenience, be classified into five types. Including spatial and temporal factors of stimulation, the energy may be varied or held constant with respect to the following attributes: (1) wave-length, (2) intensity, (3) spatial distribution on a photosensitive surface, (4) temporal sequence of stimulation, (5) a combination of the previous divisions: the *displacement* of a *pattern* of luminous energy of a certain wave-length and intensity, occurring at a given *speed* (temporal sequence). Variations in these interdependent categories may be correlated with changes in visually-determined behavior, as shown, for example, in Korte's laws for apparent movement (see Koffka, **32**).

Accordingly, a visual moving stimulus may be defined as involving the displacement of a pattern of luminous energy on a photosensitive surface dependent, as has been pointed out, on the spatial and temporal relations of such displacement. It immediately follows from this definition that differential responses may be based on three types of displacement: (1) absolute velocity of displacement, (2) relative velocity (in relation to other movements in the visual field), (3) direction of displacement with either relative or absolute velocity.

Perception of these stimulus differences by animals may be defined as a judgment made by the experimenter on the capacity for differential response or discrimination by means of overt orientation movements of the body with reference to

the stimulus (avoidance-approach reactions) and, when dealing with human subjects, the added possibility of verbal or other symbolic discriminations.

B. Investigation of the Capacity of Normal Animals for Movement Discrimination. (1) *Qualitative Observations.*—Although previous investigators have noted the great significance of visual moving stimuli in the responses of animals, little or no quantitative data bearing on this aspect of vision can be found. Exner (**15**) speaks of the ability of flies and butterflies to respond to moving light patterns. Similarly, Bell (**3**), working with the crayfish, found that they did not respond to stationary visual stimuli but reacted quickly and with precision to a moving stimulus.

The work on visual acuity measurements with insects (Hecht, **25**; Hecht and Wolf, **23**; Hecht and Wald, **24**) also has a bearing on movement discrimination, since it was found that a moving stimulus was necessary to cause these animals to respond consistently. Hecht and Wolf, in determining the visual acuity of the honey bee under different conditions of illumination, found that the bee would give definite body-orientative responses to any displacement of the visual field within the lower and upper thresholds of capacity to discriminate. Black and white striped gratings were moved by hand. The bee responded by a "reflex, sidewise movement of the head and thorax," opposite in direction to the direction of the movement of the visual field. This same technique was applied by Hecht and Wald in repeating visual acuity measures on the fruit fly. Pasche (**44**), who measured visual acuity in amphibia, concluded that the stimuli must be in motion before the animals give any adequate index of perception. Basler (**2**) found that movement sensitivity was greater than visual acuity (determined by the minimum separable method) for the human fovea.

A moving stimulus was found by Beniuc (**4**) to be effective in bringing about discriminatory behavior in fighting fish (*Betta splendens Regan*). He trained the animals to approach tweezers holding food when a revolving, sectored black and white disc was presented. The fish were induced to swim

away from the feeding place when a grey disc was presented by striking them lightly with the tweezers.

To determine whether or not white rats were capable of discriminating a moving from a stationary light, Reeves (56) attempted to train rats to give differential responses to a small lamp, swinging in an arc of three inches, as contrasted with a stationary lamp. The stimuli were located directly above the food boxes. An average of 700 trials was required by the animals to learn the discriminative response.

Szymanski (64) failed to find true discriminatory behavior in a dog with visual moving stimuli. The stimuli consisted of two white circles, joined by a cross bar, placed above the food cage. The animal was trained to go to the cage above which one of these pairs of circles was not revolving. The moving (wrong) stimulus revolved through one complete turn in two seconds. Szymanski eventually found that this dog was not using visual cues and concluded that this animal "is incapable of making fine optical discriminations."

(2) *Quantitative Determinations of Movement Thresholds.* Within the range of physical velocity of displacement of a pattern of luminous energy, two major phenomena are common to animals capable of movement discrimination, namely, the upper and lower thresholds of differential response to velocity, which may be said to be analogous to similar thresholds of wave-length and intensity discrimination. As the physical velocity of movement is slowly decreased, a value in physical units is reached below which discrimination, judged by a suitable criterion, ceases. Similarly, by increasing the speed of the stimulus, a velocity is reached above which the animal gives no sign of perceiving movement. A stationary stimulus-pattern, dependent upon the spatial characteristics of the moving stimulus, is seen by the eye under these conditions. These two types of quantitative measures of movement perception, along with those secured by using two or more stimuli moving at different speeds (relative movement discrimination), make possible accurate determinations of the sensory capacity of animals for movement discrimination.

It has been repeatedly emphasized in the literature on

human perception that the physical conditions under which experiments are performed are extremely important in the weighing of results. In no field is this statement truer than in the threshold determination of visual movement discrimination. Brown (9, 10, 11) concluded from a series of threshold measurement of real movement (lower, upper and relative) in human subjects that it is possible to predict thresholds on the basis of the physical characteristics of the stimulus situation. One threshold was experimentally determined with a given set of physical conditions. When these conditions were changed in known proportions, he found that he could predict the corresponding thresholds within small limits of error as determined by subsequent experimentation.

At least eleven primary variables have been found to affect the limits of movement perception. Briefly, they are: (1) physical velocity of the moving stimulus, (2) form and size of the stimulus, (3) characteristics of the path of movement, (4) illumination of 'figure' and 'ground,' (5) monocular or binocular observation, (6) light or dark adaptation, (7) distance of observation, (8) color of figure and ground, (9) peripheral or macular observation of the stimulus, (10) eyes allowed to move or fixation, (11) duration of the observation period. This dependence upon a multitude of factors necessitates rigorous control and complete report of every detail connected with the experimental method. Undoubtedly these considerations are operative to a large extent in the visual perception of movement of lower animals, although detailed experimental proof is not at hand.

Since threshold measures are affected by so many factors of the experimental situation, the writer has prepared Table I to attempt to include pertinent data as well as threshold values on a number of representative laboratory animals.

(*a*) *Lower Thresholds of Movement Velocity Discrimination in Animals*

Aubert (1) credits Porterfield in 1759 with the first relatively accurate description of the limits of the human visual mechanism in perceiving movement velocity. The latter

states (quoted from Aubert), "An object moving with any degree of velocity will appear at rest if the space it runs over in a second of time be to its distance from the eye as 1 : 1400." Aubert calculated the threshold from this data and found it to be in the neighborhood of 2′27″ per second angular velocity. (Angular velocity equals the angle whose tangent is the linear velocity divided by the distance from the eye.)

Aubert's own experiments on the lower threshold of the 'movement sensation' were carried out with more controlled conditions. He used a rotating drum with a clockwork mechanism which enabled him to control the speed of revolution very accurately. The detailed results of his experiments are presented in Table I.

Grim (**19**) attempted to construct a device which would "eliminate inferences of movement from mere change in posi-

TABLE I

MOVEMENT THRESHOLDS

Experimenter	Date	Material	Thresholds	Conditions
			Lower Thresholds	
Aubert..	1886	Humans	48.5–55″	'Daylight' illumination. 5 types of stimuli. Fixation. Dist. of obs. 1150 mm.
Grim ...	1911	Humans	Dark adap. 9′–13′30″ Light adap. 3′22″–8′54″	Small point revolving in circle of diam. 3 mm. Monocular observation.
Sälzle...	1932	Dragonfly larvæ	.95–2.8 rev./sec. at .043–.126 meter candles	Disc with point of light, .4 cm. diam., revolving in circle of .625 cm. radius. Dark adaptation.
Hawley and Munn	1933	Rats	200–500 cm./min. surface speed	Modified Yerkes-Watson apparatus. Vertical black and white striæ.
Kennedy and Smith	1935	Cats	2.6–14.6 degrees of central angle/sec.	Revolving crosses 9.5 cm. radius in lever discrimination boxes.
			Upper Thresholds	
Bourdon	1902	Humans	Direct: 27 full excursions/sec. Peripheral: 23	Fixation. Horizontal movement of white parallelogram on black ground. Excursion 40 mm.
Cermak and Koffka	1921	Humans	$e = a \log i + b$ where e = time of passage, i = illumination and a and b are constants	Sectors of 56 and 48 degrees fused by rotating lamp. Fixation and dark adaptation.
Sälzle...	1932	Dragonfly larvæ	Disc: 51.9–56.8 rev./sec. with .536–.626 meter candles. Cylinder: 59.73 impressions/sec.	Disc described above. Cylinder with perpendicular black and white striæ 27 cm. in diam. 8, 12, and 24 stripes used.
Beniuc..	1933	Fighting fish	100–120 impressions per second	Sectored colored discs and grey discs. Discrimination technique.
			Relative Thresholds	
Bourdon	1902	Human	Standard: 27 mm./sec. Weber Ratio 1/8 83 mm./sec. 1/12 176 mm./sec. 1/12	Two white discs with black rect. 9 by 12 mm. at periphery. Revolved in circle of 112 mm. radius. Observation distance 2 meters.
Pattie and Stavsky	1932	Chicken	Standard: 1 rev. in 7 seconds. 1 : 2.15 ratio discriminated	Vertical black and white striæ 2.5 cm. wide. Modified Yerkes-Watson box.

tion of the stimulus." The conditions of distance from observer, and size of stimulus were changed three times and measurements of the movement threshold were repeated under light and dark adaptation (see Table I).

Until Sälzle's article (**59**) appeared, there had been no attempt to measure accurately the lower threshold of movement velocity discrimination in the sub-species of animals. This author, who was interested in extending the work of Exner (**15**) on the compound eye in relation to movement discrimination, performed well controlled and ingenious experiments on dragon-fly larvæ in determining both the upper and lower thresholds. These animals react to the displacement of stimuli in their visual field by quick movements of the 'grasping mask,' with which they attempt to seize and hold the moving stimulus. Sälzle has called this activity the 'Schnappreflex' since it has the predictability and precision of a true reflex. The conditions under which it is invariably present are: (1) the animal must be hungry; (2) the stimulus must be moving and within the range of the 'mask.' Turning of the head and body, so that the stimulus is kept along the saggital axis of the body, combined with the 'Schnappreflex,' furnish the experimenter with adequate data on which to judge the animal's perception of movement.

The stimulus (see Table I) consisted of a small illuminated spot revolving in a circle before an animal restrained in an aquarium. The lower threshold, which was found to vary with the illumination of the field, was taken as the speed of movement of the dot at a given illumination which just failed to elicit discriminatory responses described above.

Hawley and Munn (**21**) used a discrimination apparatus in which albino rats were trained to choose between a pattern mounted on a moving kymograph drum and a similar pattern placed on a stationary drum. The patterns were black and white perpendicular stripes, 1.9 cm. in width. If the animal approached the stationary drum, it received an electric shock and was not allowed to enter the food box. These authors succeeded in training six animals to discriminate between the two patterns when one was moving horizontally at a linear

velocity of 1500 cm./min. An average of 758 trials for each rat was required before a level of 85 per cent correct choice in 200 trials was reached. With slow reduction of the speed of the moving stimulus, the threshold of 2 animals was found to be approximately 200–250 cm./min. A third failed to discriminate at a velocity of 500 cm./min.

The lower thresholds of real movement velocity discrimination in four cats have been determined by Kennedy and Smith (**28**). The stimuli were shadows of crosses, 9.5 cm. in radius, projected on illuminated backgrounds. The animals were trained to obtain food by depressing a lever located at the front of the discrimination box, on the door of which the revolving cross was projected. The animals received an electric shock and obtained no food when making an incorrect choice. Under the conditions described in Table I, thresholds for the four animals were respectively 2.6, 3.0, 5.5 and 14.6 degrees of central angle per second. The response to an initial speed of 80 degrees per second was established in 200–480 trials, when a criterion of 90 per cent correct choice in 40 trials was employed.

(b) Upper Thresholds of Real Movement Velocity Discrimination in Animals

Rapid real movement, as a stimulus, is closely related to flicker, with the exception that, in the case of the latter, the alternate interruption of light and dark may take place without physical displacement of the source. Both of these types of stimulus variation give rise to the phenomenon of fusion. In the case of fast movement, the animal can not discriminate between a given stationary pattern and an equivalent fused pattern at the same brightness level. In the same manner, when the time between interruptions of light reaches a certain small limit, discriminations can not be obtained between a fused and a non-flickering stimulus. Although the relation between displacement and interruption is obvious, the present paper will treat only the former. Brown (**11**) states that the same laws do not apply to both types of stimulation for human subjects.

The investigation of Sälzle (**59**) on the lower threshold of movement velocity discrimination with dragon-fly larvæ has been described above. With the same technique, this author also found upper thresholds. Under varying conditions of illumination, the 'Schnappreflex' just failed to appear at from 51.9 to 56.8 revolutions of the lighted spot per second. Using a revolving drum with 8, 12 and 24 alternate black and white stripes, he found the fusion speed with all three spacings to be 59.72 impressions per second.

Beniuc (**5**) determined the fusion velocity of real movement for fighting fish by applying the same methods described in an earlier section. A grey disc and discs with alternate black and white sectors served as positive and negative stimuli in the training of the animals. The fusion threshold was found to occur at from 100–120 impressions per second for the three fish used.

Bourdon (**7**) constructed an ingenious apparatus for translating rotary movement to linear movement. It consisted of a horizontally revolving drum with white stripes pasted at an angle of 45 degrees on the surface. The drum was viewed through a horizontal slit in a screen so that the stimulus presented to the observer was a small white parallelogram moving back and forth in the field. Under the stimulus conditions presented in Table I, Bourdon's upper movement threshold for foveal vision was 27 total excursions of the point per second and for peripheral vision 23 excursions per second.

Cermak and Koffka (**13**) carried out a precise series of experiments on the fusion of real movement for the human eye in order to ascertain whether or not Korte's well-known laws for apparent movement were valid also for real movement. A lamp, attached to a rotating arm of an electric motor, was viewed through a screen containing slits (sectors) of various sizes. The fusion speed was determined at eight stages of illumination with sectors of 56 and 48 degrees. When the size of the arc was held constant, the speed necessary to 'fill' the space with non-interrupted light varied from .9–1.2 rev./sec. as the illumination was changed over a range of 1–453 (relative units). When the illumination was plotted

against the speed required for fusion under these conditions, a logarithmic curve of the form $e = a \log i + b$ was found, where 'e' is the time of passage of the lamp, 'i' the illumination and 'a' and 'b' are constants.

Similar curves were found by Sälzle (**59**) with dragon-fly larvæ for fusion of real movement in relation to intensity of illumination. On the basis of the curves found in these studies, it seems justified to suggest that fusion of real movement depends on much the same functions as Hecht (**25**) has found in the study of visual acuity and flicker in relation to the photochemical activity of the receptor.

(*c*) *Thresholds of Relative Velocity Discrimination in Animals*

The term 'relative' movement will be used in the present discussion to designate discrimination based on 'faster than' or 'slower than' relations between two or more moving stimuli. Relative movement thresholds may be determined with one stimulus at a standard speed and the other or others approaching the standard from either a larger or smaller physical velocity. Investigations of the latter type of threshold have been inadequately dealt with in lower animals.

The ability of human observers to give a correct report of the activity of a light moving in relation to a still light was investigated by Carr and Hardy (**12**). The two lights were the only sources of stimulation in the visual field. In the experiment the intensity of the two lights, their length of path, and their speed of movement were varied. The conclusion was made that the factors of the situation which increase accuracy of report were: (1) difference in size of the lights, (2) increase in combined area, (3) inequality of brightness, (4) faster rate of the moving light, (5) fixation of the stationary light. The amplitude of the movement was found to have little or no effect. With the same apparatus and technique, Thelin (**66**) performed a more complete experiment in which both lights moved. The speeds of the lights of 2 cm./sec. and a length of exposure of 2 seconds were held constant while the amplitude and the intensity were varied. This observer concluded that displacement to the left brought

about greater accuracy of report than displacement to the right, a fact which was explained on the basis of reading habits. Changes in the absolute intensity of the lights had little effect on this discriminative behavior. These two experiments emphasize the major rôle of the many physical variables in any experimental study of movement perception.

Bourdon (**7**) used two white discs with small black rectangles located at their borders in determining the limits of his own eye for relative movement discrimination. The difference limen at three standard speeds (27, 83 and 176 mm./sec.) was found to be one-twelfth at the two higher speeds and one-eighth at the slower speed.

Szymanski (**64**), whose work has already been cited, attempted to train a dog to discriminate between circles revolving at 1 revolution in 29 seconds and similar stimuli at 1 revolution in 4 seconds. After 56 trials (days), the animal showed no signs of responding differentially. However, Tellier (**65**), in an experiment on *Macacus rhesus* and *Macacus sinecus* monkeys, found that the latter could discriminate between two revolving stimuli differing only with respect to velocity. This ability was demonstrated at three different standard speeds.

Pattie and Stavsky (**45**) succeeded in training two bantam chicks to discriminate differences in velocity. The animals were first trained to differentiate striæ moving at 1 revolution in 7 seconds from stationary striæ. One animal accomplished this in 320 trials and the other in 290 trials. After the initial response to movement had been established, the positive stimulus was made to move at 1 revolution in 7 seconds and the negative at 1 revolution in 32 seconds. The animals required respectively 60 and 120 trials in order to learn the modified situation. To test the validity of the 'Strukturfunktion' for visual movement discrimination, the speed of 1 revolution in 7 seconds was then made the negative stimulus and 1 revolution in 2 seconds the positive stimulus. The animals immediately discriminated correctly and continued to do so when the previous speeds were again employed in a retest series.

After the main experiments were concluded, an attempt was made to find the threshold for differential movement velocity discrimination with one of the animals. A speed of 1 revolution in 7 seconds was taken as the standard and the speed of the slower stimulus (1 revolution in 32 seconds) was gradually increased. The chick was able to differentiate 1 revolution in 15 seconds from the standard but failed at 1 revolution in 10 seconds.

(d) The Discrimination of Direction of Movement in Animals

Different directions of displacement of luminous energy across a photosensitive surface may also serve as stimuli to produce differential responses. Although this aspect of movement perception has never been adequately studied, yet it might be instructive to describe some of the work which has been done.

Stavsky and Pattie (**63**) trained chickens, under the same stimulus conditions as described in the last section, to discriminate right-left movement from movement in the opposite direction. Both stimuli revolved at a surface speed of approximately 40 cm./sec. Four five-week chicks were trained to respond positively to the right-left movement until a criterion of 30 perfect consecutive trials was fulfilled. The animals required between 320–580 trials before this level of response was reached.

The visual acuity measures on insects have been criticized by Graham and Hunter (**18**), who state that the insects were responding to the direction of the movement of the stimulus instead of the discreteness of the striæ. They believe that direction of movement, in terms of the photochemical reactions of the receptor, is a 'temporally determined process,' while discreteness is a 'spatially determined process.' In their own experimental work with human subjects, two arrangements of striæ were used. Thresholds for direction of movement (gratings with vertical striæ moved by hand) and discreteness (direction of lines, using the same gratings with the striæ either vertical or horizontal) were determined. For correct report on direction of movement, greater illumination

was necessary. Hecht and Wald (**24**) believe that this study is vitiated, due to the fact that the gratings were moved so fast that fusion resulted.

Thresholds of direction of movement found by reducing the speed of the stimuli have never been determined, to the writer's knowledge. If the distinction 'temporally' determined and 'spatially' determined has any meaning in terms of differences in behavior or differences in stimulus conditions, then it should be possible, by employing this threshold, to determine the sensory capacity of animals with respect to the 'temporal' type of stimulation and to compare the results with those found by the typical 'spatial' processes of stimulation.

(3) *Apparent Movement Discrimination in Lower Animals.* Two different studies suggest that fish 'see' apparent movement if the same response to real movement and apparent movement under optimal conditions is taken as the criterion. Gaffron (**16**), using a revolving-drum technique, concluded that dragon-fly larvæ and common house-flies, although they gave differential orientation responses to real moving stimuli, did not show evidence of capacity for apparent movement discrimination. Minnows and sticklebacks, on the other hand, gave definite responses to the spatially discrete stimuli of apparent movement. Conclusive evidence that apparent movement may be responded to as actual movement by minnows is presented in a discrimination experiment reported by von Schiller (**61**). The fish were trained to swim toward the correct visual stimulus to obtain food. Spots, moving vertically, served as stimuli in the experiment. A screen, which was moved by hand, presented the animal with a choice of two stimuli, one of which actually moved. It was found that the animals could discriminate simultaneous and successive phases of apparent movement from real movement but could not maintain the discrimination when time relations of the 'apparent' stimulus were adjusted to the optimal stage. These experiments suggest that, as a stimulus, apparent movement may be looked upon as a limiting case of real movement and that the ability to respond in the same way to the

reduced-cue stimulus situation of apparent movement merely demonstrated the capacity for part-whole learning.

C. Summary.—It has been pointed out in the present discussion that a visual moving stimulus is capable of evoking constant forms of behavior in all organisms possessing differentiated photosensitive surfaces. Furthermore, with suitable apparatus and training techniques, accurate, quantitative descriptions of the visual capacity of animals may be obtained by means of the upper, lower and relative thresholds of velocity discrimination, as well as those of direction of movement. The ease and precision with which the sensory limits of the visual apparatus may be determined makes available to students of vision an additional technique by means of which the nature of function in the visual system may be investigated. Finally, it is possible, by applying psychological, physiological, and anatomical techniques, to determine the physiological basis of movement discrimination in the characteristics of the receptor, the primary visual fiber projection centers of the central nervous system, and the effector mechanism.

II. The Physiological Basis of Visual Movement Perception

It was suggested in the introduction to the present paper that an adequate physiological theory of movement perception must be based on the experimentally determined knowledge of the structure and function of the visual mechanism. The psychological experiments cited previously, and others on the movement after-image and the phi-phenomenon, set a number of seemingly insurmountable problems to be solved before the physiological basis of visual movement perception may be agreed upon. Brown (11) states, "Of late in the investigation of movement perception there has been increased insistence on the development of a physiological theory that will explain all the facts of seen movement. It is very questionable for the immediate present if this can be done. But at least we can point out that when that theory does come it must explain both real and apparent movement, the apparent increase in

the number of moving objects, the threshold for fusion and the impression of duration gained by watching things in motion. Vice versa, of course, a really satisfactory physiological explanation of any one of these phenomena will bring the explanation of the others along with it." It must be remembered, however, that the psychological approach to these phenomena has done no more than set problems. For any theory of movement perception, anatomical, physiological, clinical and psychological methods and results must be taken into account.

A. Experimental Investigations concerning the Function of Various Parts of the Brain with Respect to Movement Discrimination. (1) *General Findings on the Structure of the Visual System.*—In psychological treatises on movement perception and the physiological mechanisms for such response, little or no mention is made of the basic anatomical ground-plan of the visual system. The result has been that theories have been erected which do not seem to be anatomically possible. Recent advances in knowledge concerning the anatomy of the visual system which are of importance in understanding movement perception will be covered briefly in the following section.

Corresponding to the spatial distribution of physical energies in the environment, the sensitive elements of the retina have a specific spatial distribution. Thus, light from a certain position in the environment falls on a definite area of sensitive elements in the retina. The spatial nature of moving stimuli is retained, since successive retinal elements are stimulated.

Not only is the spatial arrangement of the stimulus pattern reproduced on the retina, but it is generally concluded from experiments on mammals (Lashley, **38**, **39**; Putnam and Putnam, **54**; Poliak, **50**; Brouwer, **8**; Putnam, **55**; Holmes and Lister, **26**; et al.) that such 'projection' is the fundamental characteristic of the visual system as a whole. Third order optic neurons from the retina make synaptic connections with cells in a definite locus in the lateral geniculate body. Some fibers pass through the geniculate body to terminate in

the superior colliculus and in the nuclei of the mid-brain. According to Lashley (**38**), this 'projection' is localized in both the lateral geniculate body and the superior colliculus of the rat's brain. Studies of Brouwer (**8**) and associates suggest that projection on the superior colliculus is maintained in the monkey. From the lateral geniculate body, fourth order optic neurons, forming the optic radiations, pass by way of the external saggital bundle to terminate in connection with cells of the visual cortex (area striata, Field 17, area OC, calcarine area) located in the occipital lobes. Here at the highest level, definite orderly projection of termination is again found. Certain writers have suggested that stimulation of a definite area of the retina will cause activity in a correspondingly definite region of the visual cortex. This statement does not mean that retinal 'figures' are transmitted to the visual cortex, although that view has been suggested (Poliak, **50**). Rather, emphasis should be laid on the fact that this orderly projection gives an anatomical basis for the discrimination of change in position of light stimulation on the retina. Photochemical time relations of the sensitive elements of the retina, and corresponding time relations in the central optic centers, as suggested above, may be looked upon as the differentials for velocity discrimination. Two fundamental characteristics of the nervous structure of the visual system, then, make possible the differential responses to moving stimuli discussed in a previous section: (1) the anatomical arrangement of the visual system, (2) the time differentials in stimulation.

(2) *The Results of Physiological Experiments on Lower Animals.* Obviously, the easiest method of determining the locus of function in a given level of the visual system is by extirpating or destroying that level by surgical means and thereafter observing the visual capacity of the organism. It is generally agreed that comparison of this capacity with the normal or pre-operative ability gives rough indication of the function of that part. Such experiments have been performed on animals in connection with studies of cortical function in brightness, pattern and movement discrimination.

(a) Summary of Experiments Utilizing Brightness and Pattern Stimuli

The general results from studies of pattern and brightness discrimination in relation to the function of the central nervous system are of interest in the present discussion since they show certain inconsistencies and because responses to moving stimuli have been considered to be 'transitional' in phylogenesis between the simple brightness functions and those of complicated pattern vision.

In an extended series of experiments on the rôle of the visual cortex in brightness discrimination, Lashley (**35**) found that rats lacking this area were able to respond consistently under conditions of low general illumination of the whole retina to elementary light-dark situations. The postoperative threshold for discrimination of brightness differences was only slightly greater than the preoperative. Marquis (**41**) reached the same conclusion with dogs, Smith (**62**) with cats, and Klüver (reported by Lashley, **40**) with monkeys.

In the case of pattern vision, determined under daylight or high-light general illumination, the controlled experiments of Lashley (**36, 37**) demonstrated that pattern discrimination was lost with complete removal of the visual area of the cortex in the rat. Observations on the daily life of animals lacking the visual cortex led to such concepts as 'cortical blindness' (Munk, **43**), and more recently 'object blindness' (Marquis, **41, 42**). Under the above conditions, the dog, cat, rabbit and monkey bump into familiar objects in their cages and into obstacles set in their path. In general, such observations have been assumed to show that pattern vision is a function that depends upon the intact visual cortex. From these experiments, performed under different physical conditions of stimulation, it has been concluded that crude light discrimination may be mediated by sub-cortical centers in the absence of the visual cortex, but that the area striata is necessary for any type of pattern vision.

Recent results of Smith (**62**) cast doubt on the validity of such assumptions as 'psychic' or 'object' blindness which appear to be independent of the capacity for discriminating

brightness differences. In testing brightness discrimination, after the removal of the area striata in cats, under conditions of high-light illumination of the retina, he found that the animals could not discriminate in simple black-white situations, whereas, under dark conditions, the post-operative thresholds were equivalent to the pre-operative. This study suggests, then, that the loss in pattern vision in daylight may be looked upon as a definite sensory decrement in discriminating brightness ratios and that such categories of 'object' blindness are not needed to explain this phenomenon. The visual cortex appears to have a differential function. It is necessary for the discrimination of brightness differences in photopic illumination and apparently unnecessary under scotopic conditions, as demonstrated with the cat.

(b) *Physiological Experiments on Movement Discrimination with Animals*

Since movement discrimination seems to be closely associated with the ability to differentiate and localize brightness contours in the field of vision, the assumption might be made that the ability to respond on the basis of moving stimuli could also be found in animals capable of discriminating brightness differences after destruction of the visual cortex. Although proof of such an assumption is at present lacking, certain investigations on mammals, which are suggestive toward further work, point to the fact that the ability to respond to moving stimuli is not completely lost after removal of the striate cortex, or, for that matter, after destruction of all neo-cortex.

Schaltenbrand and Cobb (**60**) observed that a decorticate cat could localize the experimenter in a semi-darkened room. They attributed this ability to movement perception, but gave no reasons as to why the reactions of the animals could not have been controlled as well, if not alone, by auditory or olfactory stimulation.

After removal of the occipital lobes in two rabbits, Van Herk and Ten Cate (**67**) observed that all forms of pattern vision were abolished under normal illumination immediately

after the operation. The animals bumped into the wire netting of the cage and obstacles placed in their path. After some time, however, they were able to avoid obstacles at some distance, even when put in unfamiliar surroundings. A conditioned response to the sight of a food-plate, moved back and forth by the hand, was established by feeding them from the plate when they approached. A previously established auditory conditioned response was used to facilitate learning, but after a short time the visual movement of the plate sufficed. Extraneous auditory stimuli were controlled. The writers concluded that a true conditioned response to moving stimuli had been established in animals deprived of the occipital lobes.

The visual reactions of a dog without the occipital lobes have been investigated by Koudrin (34), who demonstrated that conditioned responses to moving stimuli and simple pattern stimuli could be elaborated after complete bilateral occipital lobectomy. These experiments were carried out in a semi-darkened room. A conditioned salivary response was established to the turning on of a light. After the visual areas were removed in two stages, reaction to light appeared on the 5th day following the last operation. Four months later, the animal responded to a moving cross presented on a screen. Further work showed that the animal was capable of differentiating between a luminous cross and a circle, thus demonstrating elementary pattern vision. The results of these experiments have been criticized by both Lashley (36) and Marquis (41) because no anatomical controls of any sort have been reported in conjunction with the observations. Contrary to the above results, Lashley (36) failed to find movement discrimination in four rats after complete removal of the visual cortex.

Partial bilateral or unilateral lesions of the striate areas have been found to have observable effects upon the ability to respond to moving stimuli. Kennedy and Smith (28) reported preliminary work which indicated that cats with partial bilateral lesions in the area striata retained the capacity to discriminate between stationary and moving visual patterns.

It was found, however, that the lower absolute thresholds, obtained before the operation, were greatly modified in post-operative tests.

Penard (**48**) found transient disturbances in the visual field after partial unilateral destruction of the area striata in two monkeys. Perimetric tests with pieces of apple on a wire were applied to determine residual visual function. On the first day after the destruction of the dorsal two-thirds of the left occipital operculum and the dorsal part of the saggital bundle (containing optic radiations from the lateral geniculate body), it was found that the animal would follow a moving piece of apple with the eyes in all portions of the visual field except the right lower quadrant. On the following day this loss was no longer demonstrable and after 3 months, only slight impairment was noticed. In the second animal, in whom the ventral portion of the left optic radiation was severed, visual defects for moving stimuli were found in the upper right quadrant of the visual field immediately after the operation. After six days, the monkey showed no visual defects. It can be said that partial unilateral or bilateral lesions in the area striata, although of interest to the anatomist and physiologist for the purpose of determining the validity of the general projection theory, are not of great importance in understanding the localization of specific functions at this level of the visual system.

The seemingly contradictory results obtained on animals involving complete destruction of the area striata have been explained by Marquis (**42**) and Dusser de Barenne (**14**) by means of the concept of 'encephalization' or 'corticalization' of function in phylogeny. The belief is held that, as the phylogenetic scale is ascended, the cortical connections of the visual system become increasingly more important for the mediation of receptor-released activity. Marquis, however, makes the additional assumption, in order to explain his results with brightness and 'object blindness,' that this phylogenetic reorganization corresponds to capacity to discriminate pattern differences. As pointed out by Smith (**62**), these assumptions are based on experiments performed

under totally different physical conditions of stimulation. A great deal of the apparent confusion in the field of localization of visual function in the nervous system has resulted from hasty generalization from experiments performed under totally non-equivalent stimulus conditions. Future research on the effect of variation of these conditions will be of the greatest value and interest in settling some of the discrepancies already apparent. Of course, adequate knowledge concerning the locus and extent of lesions is also an extremely important factor in the weighing of results.

(3) *Clinical Observations on Patients with Lesions in the Occipital Lobes.* The conclusions of clinical neurologists on retained capacity for movement discrimination in human subjects with injuries in the occipital lobes should be looked upon as tentative, since it appears that adequate material and methods have seldom been used. The difficulties inherent in the clinical material and technique have been covered in detail by Klüver (**30**), Piéron (**49**), and others. Since the majority of cases on which observations of movement perception have been made have resulted from the chance wounds of war, clear-cut evidence as to extent and locus of pathological effects is often difficult to obtain.

Goldstein and Gelb (**17**) studied the visual reactions of a 24-year-old soldier who had lesions in the lower and medial portions of the left occipital lobe and in the cerebellum. Perimetric examination of the vision of the patient showed serious bi-temporal shrinkage of the field with retained macular vision.

Under conditions of normal high illumination, the patient could not discriminate (verbally) movement but merely change in position of the stimulus pattern. If the experimenter moved his hand at a distance of 1 meter from the eye of the subject, it was reported that the hand appeared 'above' and 'below' but the subject did not report the 'movement sensation.' If the hand was moved slowly, the patient could fixate it and continuous following movements of the eyes were seen. The patient still reported that he saw single isolated positions of the hand. These results were also obtained when

the subject observed the moving hand of a stop-watch. The patient knew that he was presented with a moving stimulus but could not see the movement.

Strictly analogous results were found with general low illumination. The experimenter moved a small pocket lamp at a distance of two meters from the patient with an excursion of ½ meter. With fast movement, as with slow movement, the patient reported only change in position.

Riddoch (**57**) concluded, after studying ten war cases of lesion in the occipital region, that "dissociations of visual perceptions are analogous to those found in disturbances of somatic sensibility in cases of cerebral injury." By using perimetric methods he was able to determine the respective visual fields for the appreciation of movement and the form of the test object. In nine cases out of ten the field for form perception was completely dissociated from the field for movement perception, a result which the author attributes to intact sub-cortical centers capable of mediating movement perception in the absence of the cortex.

The main results of this relatively extended study show: (1) during the recovery of visual capacity after partial occipital lesion, movement and brightness are the first stimuli perceived; (2) recovery of the ability to discriminate movement begins in the periphery of the visual field; (3) "appreciation of movement and recognition of an object are always dissociated in patients in whom recovery of vision can be demonstrated; the field for the former, which is the more primitive perception, being the larger."

The exact nature of the recovery process after partial lesions is not known. Whether the sub-cortical visual centers are capable of subsuming the normal function of the visual cortex or whether remaining portions of the cortex function 'vicariously' is still a matter of debate. Vicarious function, on the basis of anatomical considerations, appears to be improbable.

Poppelreuter (**51**) tested visual movement sensitivity of a patient with macular and perimacular amblyopia from a gunshot wound through the back of the head. He divided the

visual field into five concentric zones running peripherally from the center. In zones A, B and C, the perception of stationary patterns was very inadequate. In zone C, however, movement perception was faultlessly retained. Although the subject could not discriminate the form of the moving stimulus, he was able to tell the direction of movement and could even differentiate simple movement forms (circles and crosses traced out with a lamp). This case confirms the conclusions of Riddoch concerning dissociation of form and movement, but is not in accord with the results of Goldstein and Gelb.

Pötzl and Redlich (**52**) reported the case of a 58-year-old woman with bilateral lesions in the occipital lobes in whom reactions to light and color were retained while capacity for movement and form discrimination were lost. If the experimenter moved a bright source of light in front of the subject, she reported that she saw "more light." She gave the same response when two or more stationary lights were presented. Word-blindness, shrinking of vocabulary, and 'inattention' complicated the determination of sensory function.

The cases covered above have been selected because they involve some attempts to test the possibility of retained capacity to discriminate on the basis of moving visual stimuli. The general conclusion from clinical studies on the function of the visual area of the cortex after partial lesions has been that brightness, pattern, movement, and color discrimination are somehow separable entities, the loss of one of which, due to lesions in the visual area, may have no effect on the others. Poppelreuter (**51**) states that brightness, pattern, and movement may be considered as 'part reactions' in that destruction may be specific for one and not for the others. Best (**6**), Pötzl (**53**), Riddoch (**57**), and in the lower animal field Marquis (**42**) and Lashley (**36**) assume a phylogenetic mechanism comparable to Head's (**22**) epicritic and protopathic schema for general body sensations, through the action of which pattern may be lost with retained differential brightness discrimination.

In keeping with the theory of 'encephalization' of function,

Marquis (**41**) covers nine cases with complete bilateral loss of the visual area in human subjects. No visual reactions, with the exception of the pupillary reflexes, could be elicited. Piéron (**49**) and Dusser de Barenne (**14**) also state that total blindness results from complete bilateral lesion in the visual area with humans.

On the basis of local cell degeneration, a few attempts have been made to localize visual function in the cell layers of the cerebral cortex. Kleist (**29**) presents evidence for the belief that the spatial visual functions (pattern and movement) are mediated by layer III of the calcarine cortex. Best (**6**), on the other hand, claims that the calcarine area is merely the locus of binocular image 'fusion' and that other cell constellations are necessary for movement perception. Goldstein and Gelb (**17**) hold to the 'whole cortex' hypothesis. Riddoch (**57**), as stated above, believes that movement discrimination may be mediated by the sub-cortical centers of the visual path. Adequate experimental data in support of these hypotheses is not yet available.

B. Physiological Theories of Movement Perception.—Many theories concerning the physiological basis of movement perception have been devised by psychologists, physiologists and clinical neurologists. Generally, these theories may be said to have a great deal in common in that they all involve the postulation of some hypothetical function at a given level of the visual apparatus. Among the different structures variously mentioned as predominantly mediating movement perception in lower and in higher animals are, for example, the sensitive elements of the eyes, eye movements, sub-cortical visual centers, the striate areas of the cortex, the entire cortex, and the visual system as a whole.

Psychological writers have been all too prone in most cases to generalize about anatomical structures and physiological mechanisms purely on the basis of psychological data, and even in some instances base their theories upon questionable analogies with physical phenomena of movement (Koffka, **31**, **33**). The physiologist, on the other hand, has been handicapped in contributing to this problem by the failure to apply exact techniques to the study of sensory

function, while the observations of clinical neurology, although extensive, are nevertheless vitiated by lack of knowledge concerning the cases investigated and by the difficulty of controlling subsidiary disturbances, such as language defects, in the investigation of visual function. Consequently little or no accurate knowledge exists concerning the physiological activity correlated with movement perception, although it is hoped that, by combining the methods of both comparative psychology and physiology in investigations of animals whose nervous structure may be modified experimentally, certain facts may be discovered which may be of significance in the general understanding of movement perception.

C. Summary.—The writer presents no theory of visual movement perception. It is evident from the preceding pages of this review that such a theory must wait for further experimental work in the above-mentioned fields.

Certain general facts, however, are emerging from the experiments so far reported. In summary, these are:

(1) Results of anatomical investigations on the visual apparatus of animals demonstrate that, in the higher vertebrates and man, spatial 'projection' is the basic ground-plan. The specific relation between this arrangement and the complexities of visually-controlled behavior is not yet worked out. Some suggestions for the interpretation of these facts have been made in the present paper.

(2) It is suggested by some extirpation experiments on lower animals that elementary movement discrimination may be mediated by sub-cortical centers in the absence of the cortical visual area, although the results thus far obtained give no quantitative index of such discrimination and merely point to the need of further accurate work. Partial lesions are consistent in showing little gross effect on this function.

(3) Clinical observations on human subjects with complete bilateral lesions in the visual area suggest that the intact visual cortex is necessary for any kind of vision. The results from partial lesions are not consistent in determining whether movement discrimination is retained or not.

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FACTORS IN SPACE LOCALIZATION DURING INVERTED VISION. I: INTERFERENCE

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I. A Critical Review of the Literature

Experiments on normal human adult subjects have not greatly enhanced our knowledge concerning the development of space localization. This is in part due to the fact that the main emphasis has been placed upon perceptual phenomena which have often been so shrouded in vague phenomenological terms as to make their existence mere artifacts. Furthermore, space localization develops so slowly in the human organism and is so interrelated with spontaneous involuntary responses and maturation processes as to resist careful analysis in the adult subject. Hence the more fruitful recent trend has been to study the genesis of orientation in animals, young children and abnormal human subjects. However, experiments with animals and children are usually tedious and are void of the subject's report. Furthermore, resulting data present greater difficulty of interpretation and in the case of animals the data yield only inferential information for human orientation. Data from abnormal subjects also present difficulties in that they must be translated into what they would mean in terms of the 'normal.'

There is another method of research which invokes only temporary abnormalities in the sensory-motor behavior of normal human subjects. This method avoids a number of the inferred difficulties. Temporary inter-sensory and sensory-motor disorientation may be induced by the use of refracting lenses, prisms and reflecting mirrors in the visual field (**35**, **37**, **14**, **45**, etc.); by pseudophones (**47**) and sound reflectors in the auditory field (**1**, **32**); by anesthetics in the cutaneous senses; by chemical and physico-chemical stimulation of the static sense (rotary devices, ear douching, etc.) (**22**, **41**, etc.); and

by the use of drugs (hashish, mescal, etc.) in the organic sense department (**24**). A careful study of the disorienting effect from such stimuli and the recovery brought about either through immunity, adaptation or learning, throws valuable light upon the growth of spatial habits and upon the automatic functions of the vestibular-motor and other sensory-motor systems. This may be illustrated in the oculo-motor mechanism. Thus eye-muscle contraction is induced by a number of stimuli. These are: (*a*) Spatial-temporal and spatial-intensity changes of light; (*b*) kinaesthetic stresses and strains resulting from (*a*) and from tonic innervations in neck and other bodily muscles; (*c*) changes of position of body with reference to gravity resulting in vestibular nystagmus and (*d*) centrally induced stresses and strains resulting from conditioning. To the stimuli in (*d*) belong the saccadic fixational innervations which are believed to have been conditioned to or superposed upon the more primitive quick phase of ocular nystagmus (**9**). Such conditioned innervations are believed to have their source in symbolic processes as Hudgins (**20**) and Ewert (**15**) have shown. Hudgins found that the pupillary reflex could be conditioned to language behavior, and Ewert found that the typical eye movements present while reading specific material were induced by oral or silent recall of the same material, thus showing inter-symbolic-motor conditioning. Since fixational eye movements in a given direction have become conditioned to body movements in the same direction and since the locality of objects is thus based upon habitual and conceptual behavior which has attained congruity through practice, it is very enlightening to evoke disharmony among the senses, reflexes and verbal concepts of locality in order to dissect their organization and operational characteristics.

Following Wundt's (**46**) experiments on prismatic vision and especially Stratton's (**35**, **37**) study of inverted vision, there have appeared many and diverse interpretations of their findings. Judging from the material in psychology text books and periodicals, the interest has centered chiefly on the sensory phase of Wundt's and Stratton's results. Recently Carr (**7**)

has given them more of a motor interpretation. However, on account of certain misconceptions such as avowed absence of eye movements during lenticular inversion and complete sensory adaptation to the visual experience of inversion, Carr has failed to recognize the true motor implications in the experimental data of distorted vision. In this paper I wish to deal directly with several disputed phenomena of distorted vision, to present evidence against the possibility of a complete sensory adaptation to the visual experience of inversion, to present experimental evidence that eye movements occur during lenticular inversion, to argue that such movements play an important role in the interference of localization during inversion and in overcoming the interference with practice, and finally, to show that, although an eye movement theory, by itself, cannot cope with all of the problems of space localization, it nevertheless emphasizes an important factor.

Experiments on distorted vision were introduced by Wundt (46) to test the adequacy of the nativist theory of space perception. Wundt says: "Let the experimenter have a pair of prismatic spectacles made whose angle of refraction, for the purpose of avoiding disturbing color effects, does not exceed 5 or 6 degrees. When looked at through such a pair of glasses, contours with straight lines appear bent, plane surfaces arched, and complicated views correspondingly distorted. If a person can make up his mind to wear such a pair of spectacles continually, these distortions disappear in a very few days. . . . Inasmuch as the image on the retina in this case [*i.e.* during distorted vision] remains the same, this elimination of the metamorphopsia can hardly be ascribed to anything else than an adaptation of the elements of the retina to the new condition of vision." (p. 514) This elimination of metamorphopsia Wundt regarded as incompatible with a nativistic theory, according to which the innate localization of the visual impressions is brought about immediately by innate, unchangeable, local signs, connected once for all with the various portions of the retina. Wundt's interpretation of his findings has been widely accepted. However, the so-called elimination of dioptric metamorphopsia has also been regarded as equally incom-

patible with the eye movement theory of space perception. Lipps (**26**) states the matter as follows: "The eye-movements, by virtue of which we apprehend the image distorted by prismatic spectacles, are determined solely by the nature of the *image*, that is, they are exactly the same as they would be if the image corresponded with reality. It is manifest that the eye-movements might correct the distorted picture, however, only if they were *independent* of the distortion, that is, if the wearer of the spectacles in spite of this distortion should move his eyes as he would in case the undistorted, normal image, or the image corresponding to reality, should replace the distorted one" (p. 57). It should, however, be pointed out that whereas eye movements during prismatic vision are not incompatible with the sensory aspects of the visual field itself, they are incompatible with the sensory and motor aspects of other sense departments, and with the innervations of bodily muscles. This is true because the refracting prisms cause the eyes to move out of line or out of 'focal centre' with reference to other movements of the body, and localizing responses guided by vision are now incompatible with those guided by other senses. Hence, as soon as other sense departments or bodily reflexes become involved in space localization, interference may be said to exist. Since localization is generally regarded as an active rather than a passive process, such interference may be said to exist for all forms of space localization during distorted vision.

Stratton (**35**, **37**) repeated Wundt's experiment with the purpose of testing the various theories of space perception (especially, the projection and the eye movement theories) and to determine whether an inverted image on the retina is necessary for upright vision. Instead of prisms Stratton used a system of lenses which inverted the visual field point to point by 180°. These lenses were worn continually, except during sleeping hours, on two separate occasions, 3 and 6½ days respectively. I shall not here give a full account of Stratton's results as these are generally known and have been reviewed elsewhere (**14**, **7**). There are, however, several disputed points that deserve mention.

1. Stratton combined two quite different problems in his study of inverted vision, namely, that of dioptric metamorphopsia, introduced by Wundt, and that of upright vision as related to the retinal image. These two problems are discussed together in a manner which suggests that they are necessarily interrelated. That is to say, Stratton sought to explain the so-called complete sensory adaptation to inverted vision by showing that inversion of the retinal image is not necessary for upright vision. The complete adaptation, according to him, is explained by the fact that the sense data from the other sense departments swing into line with the new visual sense data during inversion as a result of practice. Ewert (**14**) performed experiments which disproved this assumption.

2. Stratton's results have commonly been interpreted as showing that complete intra-sensory as well as inter-sensory and motor adaptation to inversion would occur with prolonged practice. Carr (**7**), for instance, says, "While a perfect stable reorganization was not attained, Mr. Stratton is of the opinion that the reorganization was so far advanced that a perfect cooperation would have been effected if the experiment had been long continued." (p. 23) Ewert (**14**) reported improvement with practice in the various forms of inter-sensory-motor activity where vision served as a distractor to space localization. However, no change was observed in the intra-sensory experience of inversion except a decrease in the frequency of attention to this phenomenon. Ewert's subjects further reported that a lapse of attention from the visual experience of inversion often creates an *illusion* of normalcy or sensory reinversion which, however, disappears as soon as the visual field is again directly scrutinized. This observation clearly supports Stratton's (**35**), as may be seen from the following quotation: "As to the relation of the visual field to the observer, the feeling that the field was upside down remained in general throughout the experiment. At times, however, there were peculiar variations in this feeling according to the mental attitude of the observer toward the present scene. If the attention was directed mainly inward, and things were viewed

only in indirect attention, they seemed clearly to be inverted. But when, on the other hand, full attention was given to the outer objects, these frequently seemed to be in the normal position . . ." (pp. 614–615). This statement clearly indicates that a lapse of attention from the experience of inversion created the *illusion* of 'normalcy' in the visual field. Although Stratton does not speak of this phenomenon as an illusion, the above quotation directly suggests it since, as Stratton points out, the 'feeling of normalcy' immediately disappeared when attention was directed to the appearance of the visual field. Such illusions are undoubtedly caused by the fact that the pattern of the total visual field is not altered by lenticular inversion. It is only when data from other sense departments and motor activity come into play that inversion becomes a reality. Hence, when attention is limited to the details of the visual pattern no 'feeling' of inversion is possible.

There are, however, a number of more straightforward difficulties in the way of complete adaptation to the experience of inversion. (*a*) Stratton's claims of marked progress in adaptation to the experience of inversion have never been verified. Negative results were obtained by Ewert's three subjects, who wore inverting lenses more than twice as long as did Stratton, and by the late Joseph Peterson who, in an unpublished study, wore Ewert's apparatus about three times as long as Stratton wore his. Peterson took special pains to repeat Stratton's experiment, *i.e.*, the behavior routine was like that of Stratton's. Brown (**6**), securing prolonged vertical inversion of 75° by means of prisms, reports that "The new perceptual situation, . . . never became quite 'natural' although during the last half of the week's experiment both the fact and the amount of inclination seemed to be very uncertain" (p. 135). This uncertainty is probably due to lapses of attention from the 'perceptual situation.' Wooster (**45**) and Gibson (**18**) also report partial but not complete adaptation. It must be remembered that Wooster (**45**), Brown, and Gibson like Wundt employed prisms to distort the visual field. In every case distortion was limited to a single plane (usually the vertical). Hence, only a part of the visual pat-

terns was distorted with reference to body and other sense departments.[1] Stratton (**35**, **37**), Ewert (**14**), and Peterson, however, employed lenses which distort the field in both the vertical and horizontal dimensions. Eye-body movements, with reference to an external object, are thus affected in all directions and the total visual pattern is involved in all localizations. For this type of inversion complete adaptation is highly improbable. At any rate, its occurrence has never been reported. Current assumptions rest upon the illusion of reinversion during lapses of attention from the phenomenon, and upon the speculation that, since such lapses increased as the novelty of inversion wore off, complete adaptation would eventually take place.

(*b*) Perception of locality is dependent upon bodily orientation. Predominant in visual orientation are accommodation and the movements of eyes and head. As I shall demonstrate more clearly below, inverting lenses reverse eye movements with reference to the perceived object. However, eye movements may also be evoked through the vestibular mechanism, by excitation of bodily muscles and voluntarily (through verbal or conceptual conditioning). Thus only the ocularly induced movements are reversed; the others are initiated as in normal vision. The result is antagonism or interference between the variously initiated movements. This type of antagonism has been generally disregarded by proponents and critics of the eye movement theory of space perception. Since eye movements of ocular, vestibular and kinaesthetic origin are known to be physiologically related to each other (see pp. 16–19), it is not probable that the experimentally-induced antagonism could be completely eliminated. Such antagonism would probably persist as in reversal of handedness, *i.e.*, as when a manual shift is made from the preferred to the non-preferred hand.

(*c*) Lenticular inversion does not cause a structural disruption of the pattern of a seen object. The intrinsic spatial

[1] Furthermore, Wundt and Gibson deal with the metamorphopsia or intra-sensory adaptation occurring within the visual mechanism itself. This is a different problem from that investigated by Stratton and Ewert, which involves inter-sensory-motor adaptation.

pattern characteristics of a seen object, *e.g.*, chair, retain their relative properties. This is due to the fact that the apparatus used by Stratton and Ewert produces a point-to-point transposition of each pencil of light in the resulting upright image of the retina. Thus if the sensory visual experiences resulting from the image are considered by themselves there is no disruption in the visual field. The only changes and resulting disturbances that take place are those that bear reference to the other sense departments, inherited and habitual localizing habits and eye movements of vestibular, tonic, and central origin. (For example, the visually perceived sky line changes position with reference to the felt body and normal localizing habits.) Hence, there is no question of a reorganization of the spatial attributes of a seen pattern, or of adjacent retinal points to each other, when the sense data related to the image are considered by themselves. *In every case the reorganization involves the co-ordination of sense data from a totally inverted visual pattern with the sense data of unaltered sense departments, with gross bodily activity both inherited and acquired, and especially with eye movements of non-ocular origin.* This explains why Ewert (14) found no interference in localization outside of the inverted visual field and also why his subjects did not overcome the experience of inversion. It further shows that space localization must be considered from the standpoint of inter-sensory-motor experiences of the total reacting organism, since direction, locality, and depth lose meaning when considered purely on the basis of intra-sensory experiences.

(*d*) Perception of locality, distance, and depth are largely dependent upon convergence, accommodation, retinal disparity, and other physiological and psychological factors. During lenticular inversion a number of these factors are reversed. Having attained a given structural interrelationship in the course of evolution, with reference to gravity and the upright position of man, these processes would undoubtedly resist a complete onto-genetic adaptation. This view, however, does not necessarily rule out such types of adaptation as may occur in the inter-sensory-motor and conceptual processes. Nor does it rule out the normal types of intra-sensory adaptation

as will be shown in my next paper. Hence, we are not forced, by virtue of this interpretation, to accept an unqualified nativistic theory of space perception.

3. Stratton (37) interpreted his findings as disproving the eye movement and the projection theories of space perception. This may be seen from the following quotation: "The experiment makes it clear that the harmony between sight and touch does not depend on the inversion of the retinal image. The spatial identity of tactual and visual objects evidently does not require that there should be a visual transposition of objects or that they should be given some special direction in the visual field. The chief reason for the existence of the projection theory is therefore taken away. Nor, on the other hand, are the visual directions made known to us and determined through our perceiving the 'absolute' or pure motor direction of movements which alter the line of sight. The facts all go to show that the direction of movements of the head and eyes is not judged on purely muscular evidence, independently of the simultaneous changes in vision itself. On the contrary, the movements are soon felt as having a direction opposite to that of the objects passing through the visual field. During the experiment, for instance, I often felt my eyes turn toward the sky and away from my feet, although they really turned toward my feet. The felt direction of the movement is, therefore, relative to the direction of the movement of the visual objects, and the 'absolute' muscular direction cuts no decisive figure in the perception at all. This will no doubt seem a hard saying to those who have been pinning their faith more and more on the unimpeachable witness of muscular sensations. It certainly makes the eye movement doctrine of visual directions of little practical assistance for understanding the harmony between sight and touch" (p. 480–481). Here we see again that Stratton deals chiefly with the conscious aspects of space perception, following Lotze and Wundt. Even motor activity is thought of in terms of 'felt experiences' and not in terms of measurable errors of localization. Thus Stratton argues that, since eye movements were reversed during his experiment and localization could be re-

learned, the felt sensations arising from the ocular muscles or the resulting feeling of effort could not be regarded as the basis of localization. With this view I fully agree. However, it apparently did not occur to Stratton that 'felt experience' may in itself be a product and not a cause of motor activity. As a matter of fact, the most important processes of space perception such as accommodation, convergence, coördinate compensatory and nystagmic eye movements, postural reflexes and others are not directly under voluntary control. Any thorough-going theory of space perception can not rest entirely upon introspective data, but must also take into account the work of Magnus, DeKlyn, Maxwell and a host of other students on involuntary postural reflexes, and the work of Dodge, his students, and many others on involuntary eye movements, and especially the various experiments on overt localization. From the standpoint of such data it becomes evident that interference in space localization does not rest mainly upon an interference of conscious processes but more directly upon a series of unconscious processes, such as tonic and reciprocal reflexes. Viewed in this light, interference in space localization during inverted vision results chiefly from the distorted visual sense data with reference to other sense data, the antagonism between the various types of eye movements, the antagonism between ocularly induced eye movements and general bodily movements, and the resulting antagonism in the central mechanism. It will thus be seen that although we cannot build a theory of visual space localization upon the phenomena of eye movements alone, such movements, nevertheless, play an important role in the localizing process.

Carr (7) has taken the viewpoint that Stratton's experiment did not disprove the eye movement theory of space perception, "because the localizing habits of the eye were not disturbed. In order to disrupt these habits and establish a system of antagonistic reactions to retinal stimuli, the inverting system of lenses must be attached directly to the *eye ball* so that the lenses will move with every movement of the eyes" (p. 29). From this quotation it is clearly evident that Carr

has overlooked the fact that eye movements are initiated not only by the retinal stimulus but also by vestibular, muscular, and central (voluntary) stimuli. However, Carr goes even a step farther in maintaining that ocularly induced eye movements are not affected by inverted vision. He maintains that a shift of fixation in the visual field is now limited to head movements. This, he believes, invalidates the statement, made by Ewert (14), that the lenses create antagonism between 'normal' eye movements and those evoked by the lenticular apparatus. He says, "In a repetition of Stratton's experiment, Ewert states that in fixating an object the eye movements are disrupted, while the head movements are not. He also states that swinging movements accompany both head and eye rotations. In our opinion, these statements are contrary to facts. . . . Perhaps the discrepancy between the two accounts is due to a difference in the point of reference from which the directions of these movements are described. We have described these movements in terms of their relation to the *object as seen*, or, better, in terms of their relation to the *retinal position* of the stimulus. Probably Ewert was describing them in terms of their relation to the objective position of the stimulating object. . . . The habits we are dealing with are retinal-motor habits—associations between retinal stimuli and localizing responses, and these habits are disrupted only when the direction of the movements in the relation to the *position of the retinal stimulus is altered*. From this standpoint, the ocular habits remain the same, while antagonistic head movements are required for fixation" (pp. 37–38). It is true that I described eye, head, and body movements "in terms of their relation to the objective position of the stimulating object." This, it appears to me, is the most logical procedure since it was only vision that was disrupted with reference to the stimulating object and the purpose of the experiment was to investigate the effect of such disruption upon space localization in the *total* organism. Carr's description of the stimulating object would regard the experimental condition or inversion as the 'normal' and normal vision as the 'abnormal' situation for the organism. However, this

difference in the point of reference to the stimulating object does not explain the real discrepancy between the two accounts.

The chief discrepancy lies in Carr's statement that "ocular habits remain the same, while antagonistic head movements are required for fixation" (p. 38). Let us now examine the nature of eye movements more closely so that we may better understand the import of Carr's statement.

Dodge (**9**) has described five major types of eye movements, namely, saccadic, pursuit, coördinate compensatory, reactive compensatory, convergence and divergence, which have been commonly accepted as useful points of departure in studies of ocular adaptive behavior. The question now arises: which of these five types of movements does Carr consider to be unaffected by the use of inverting lenses? In order to answer this question let us briefly consider the genetic development, the interrelationship, and the conscious control of the various movements.

McGinnis (**29**) found that, "Both the large saccadic eye movements, which make up the quick phase of optic nystagmus, and the slow, gliding, pursuit movements (composed of several small saccadic movements), which make up the slow phase, occur during the first occurrence of optic nystagmus" (p. 416). Optic nystagmus was found 12 hrs. after birth and even in an infant who was born one month prematurely. A few coördinate compensatory movements were present a few days after birth. Ocular pursuit, however, was not exhibited during the first two weeks of life. Apparently, the quick phase of vestibular origin and the slow phase of ocular origin, which together compose nystagmus, and possibly coördinate compensatory movements, may be considered as constituting the nucleus or the pattern upon which the other types of eye movement, described by Dodge, later develop through practice. If this interpretation is accepted, it follows that the only type of eye movements of voluntary origin, namely the saccadic, is inextricably related to movements of vestibular or involuntary origin. Such interrelationship is supported by the following quotation from Travis and Dodge (**42**). "Dodge found [**10**] that photographic records of ocular nystagmus,

whether evoked by successions of moving objects or by rotating the head with the eyes closed, were almost identical. The essential difference seemed to be the manner of excitation and the latency and persistence of the nystagmus. The slow phase of vestibular nystagmus arises from the stimulation of the semicircular canals and involves only subcortical pathways, beginning after a latency of the order of .06 sec. The other type arises from stimulation of the retina by the moving object and involves, in addition, higher cerebral pathways, beginning after a latency of the order of 0.20 sec." (p. 99). In support of the view that saccadic movements alone are of voluntary origin, we may cite another quotation from Travis and Dodge (**42**). "The only type of eye movement which is ordinarily initiated voluntarily is the saccadic, or the refixating movement in optic nystagmus, and once begun even this seems to pass from voluntary control" (p. 101). In answer to the question as to which type of eye movement Carr believes to be unaffected by lenticular inversion, we may infer, in the face of the above cited facts, that he is speaking of the saccadic type. He may also have in mind movements of convergence and divergence, since these are partially under voluntary control and are dependent upon retinal stimulation. However, since the various types of eye movement have been shown to be of different origin, lack of retinal stimulation would not necessarily obviate all other types of movement, *i.e.*, even if retinal stimulation were lacking movements of vestibular origin would still persist. We shall, however, show later on in this paper that even those types of movement which are dependent upon retinal stimulation are altered by lenticular inversion. That nystagmic eye movements of vestibular origin play an important rôle in the maintenance of bodily equilibrium and in the various eye-body coördinations is no longer questioned. A splendid summary of the so-called tonic or reciprocal intersensory-motor reflexes, originating in the static sense, has been made by Dusser De Barenne (**13**).

Dusser De Barenne's summary also contains an extensive bibliography, including references to the most important investigations in this field. It has been shown that reflexes

affecting the eyes affect also the entire body. Thus it would seem that with each tendency toward eye movement there exists a tendency toward reaction of other bodily muscles. The effect upon space localization is clear, *i.e.*, with the impulse to move the eyes in a specific direction there simultaneously arise orientation innervations in various muscles of the entire body. The innervating effect of eye movements upon the muscles of the neck may be consciously detected. Bartels (2) reports that "eye movements without head movements ordinarily extend only 12°; if eye movements are greater there result also head movements. Uncomfortable inhibitory sensations result in the neck muscles if one voluntarily moves the eyes more than 12° without head movements" (p. 281). The innervating effects between eye and body muscles appear, furthermore, to be reciprocal. Magnus (27), Kleijn (22, 23, 43), Dusser De Barenne (13), Radimaker (33), and others have shown that neck and labyrinth reflexes also initiate eye and body movements. It is thus clear that experimental facts of inter-sensory-motor reflexes must play an important role in the interpretation of space orientation. It is also clear that the absence or presence of ocularly induced eye movements during distorted vision is of utmost importance to the interpretation of experimental data and previously proposed theories of space perception. Let us now attack this problem more directly.

II. Inversion Lenses [2]

In view of the fact that the occurrence of eye movements, in the absence of head movements, during lenticular inversion has been questioned it is essential that we should examine the nature and function of the lens systems used for inversion experiments. In 1930, Ewert (14) reported some technical improvements upon an apparatus first used by Stratton (35) for prolonged inversion experiments. However, the basic principles of the two apparatuses are essentially alike. Stratton did not report the focal length of his lenses. Hence it will be impossible to compute the position of the image formed by

This section has been prepared with the help of Prof. L. A. Woodard of the Physics Department at the University of Vermont.

his lenses or to draw to scale a beam of light traversing them. However, the position of the image does not determine the presence or absence of eye movements. *From the standpoint of eye movements the lenses used by Stratton and Ewert function essentially like ordinary spectacle lenses within the respective range of the visual field employed.* Nevertheless, to make this point clear it will be necessary to produce measurements of the focal length, diameter, thickness and locations of the focal points of the lenses used by Ewert, and then present a diagram, drawn to scale, which traces rays of light through the lenses. Since an extensive description of the apparatus has been previously given (14), I shall repeat here only the essential features.

The lens system held in front of each eye a light aluminum tube. These tubes were adjustable for interpupillary distance, and the apparatus as a whole was adjustable for comfort while wearing. A cloth contact with the head prevented any stray light from reaching the eye. The inverting apparatus consisted of three plano convex lenses furnished by the Spencer Lens Co. These lenses we shall designate A_1, A_2 and A_3, with A_1 being the eyepiece or lens nearest the eye, and A_3 the objective or lens farthest from the eye. The plane face of A_1 was toward the eye, and the plane face of the other two were toward the entering light. The following table gives, for each

TABLE I

	Lens A_1 (eyepiece)	Lens A_2	Lens A_3 (objective)
Focal Length (Spencer Lens Co.)	30.0 mm.	38.0 mm.	51.0 mm.
Diameter (measured)	16.0 mm.	28.0 mm.	20.0 mm.
Max. Thickness (measured)	3.4 mm.	6.6 mm.	7.2 mm.
F to near face (measured)	32.2 mm.	37.6 mm.	42.3 mm.
F′ to near face (measured)	30.0 mm.	41.0 mm.	48.5 mm.

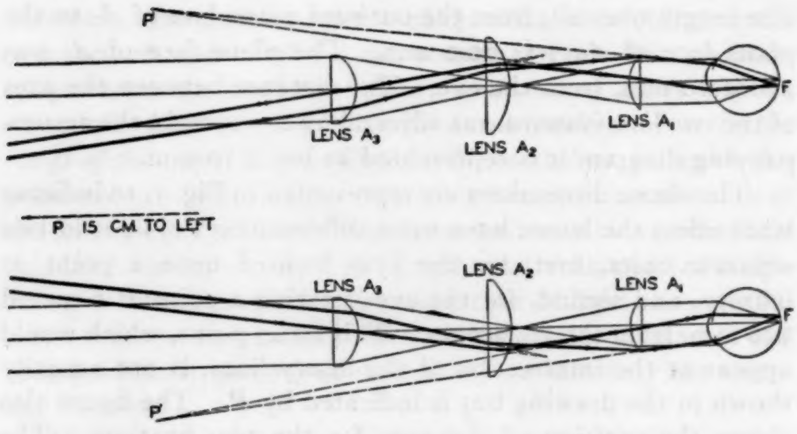
lens of either system, its focal length, diameter, and thickness, and the locations of its focal points as given by the Spencer Lens Co., or as determined from the experimental location of the images of distant objects.

The intervals between the lenses were approximately equal. The length over all, from the outward plane face of A_3 to the plane face of A_1, was 90.0 mm. The plane face of A_1 was about 10 mm. from the eye. The distance between the axes of the two lens systems was adjustable; as shown in the accompanying diagram it is represented as being 70 mm.

The above dimensions are represented in Fig. 1, to indicate what effect the lenses have upon different rays of light in two separate cases, first, for the eyes focused upon a point at infinity, and second, for the eyes fixating a point P situated 250 mm. from the apparatus. This latter point, which would appear at the intersection of the heavy lines, is not actually shown in the drawing but is indicated by P. The figure also shows the position of the eyes for the two fixations. The individual lenses are thus drawn to scale, and the intervals between them and between the apparatus and the eyes are drawn to the same scale. The focal points of the lenses are not shown in the diagram, with the exception of the secondary focal point of the objective, and the primary focal point of the eyepiece. The formation of images was experimentally investigated. An auxiliary lens was mounted on the optical bench, and a pair of conjugate object and image points was obtained. The lens system was then placed immediately in front of the auxiliary lens and the resulting new image was located. From these known abscissae, the position of the new object point for the auxiliary lens, or the image formed by the lens system, was calculated. One of these pairs of conjugate points consisted of a distant object and its image. In this case an image of the distant object was formed by the lens system at a position about 30 cm. from the point where the eye would be when the apparatus was worn. In a similar manner it was determined that with objects 50 cm. from the apparatus, the eye would be observing images only 17 cm. from the eye. On the basis of these experimental determinations, the following rays were drawn.

The light lines in the figure represent two bundles of rays, one from each eye, coming from a point at infinity. They are shown as entering the eye after traversing the lens system and

FIG. 1.



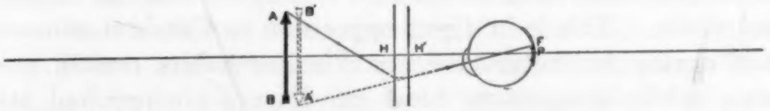
are brought to a focus upon the fovea. These two focus points are also represented as being on the axes of the lens system. The heavy lines in the figure represent two bundles of rays coming from point *P*. The images, formed by the two lens systems, are labeled *P'*. These images are virtual, and are about 15 cm. from the eye. Their rays enter the eye as coming from the direction *P'*, and are then brought to focus upon the fovea *F*. The eyes are represented as being turned so as to fixate point *P*, *i.e.*, so as to receive the rays from *P* on the fovea. It should be noted that whereas the eyes would have to converge in order to fixate point *P* in normal vision, they are now forced to diverge to fixate this point. This undoubtedly explains, in part, the pseudoscopic effect produced by this apparatus (14, p. 200). The change of eye lens necessary for accommodation is not shown in the diagram. It, however, is essentially the same as for normal vision.

From a close examination of Fig. 1, it is then apparent that the visual field as seen by an observer wearing the apparatus was inverted, slightly magnified, and consisted of images located approximately 17 cm. from the eyes. Now the question arises: what effect did the apparatus have upon the movements of the eyes while fixating various points within the circular visual field of 34.5 (see Ewert's monograph) which this apparatus permits? The following effects were produced upon

the eyes while the head was held in a fixed position. (1) The images on the retina were 'erect,' *i.e.*, to fixate objects above the line of sight, the eye had to rotate downward, or to fixate points to the left of the line of sight, the eye had to rotate to the right, and vice versa. (2) The accommodation for distance was normal, *i.e.*, a shift from near to far fixation produced the same kind of change in the eye lens as in normal vision, although not of the same degree. (3) Binocular convergence was reversed, *i.e.*, in order to shift from far to near fixation the eyes had to be turned outward rather than inward. (4) When head movements occurred, they were in the same direction as in normal vision. That is to say, to make the point fixated the center of the visual field, or to direct the visual field to a point not already included in this field, the necessary head movements were in the same direction as without the use of the apparatus.

These effects are shown schematically in Figs. 2 and 3.

Fig. 2.



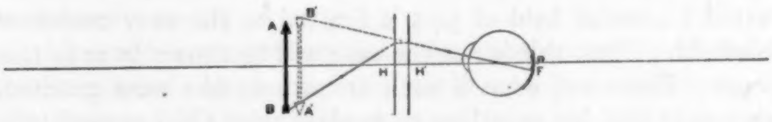


Fig. 3.

An object is represented by the arrow *AB*. The lens system, for one eye, is represented by the unit planes *H* and *H'*. The virtual image, *A'B'*, which is produced by the lens system, is seen as inverted, nearer the observer, slightly magnified, and is represented as filling the entire field of vision. The image on the retina is erect. When the eye fixates the central point of the field, the fovea is at *P*. Now if the eye is to fixate point *A*, the top of the arrow, the eye must be rotated down-

ward so as to receive upon the fovea that beam of light which comes from *A*. When, on the other hand, the eye is to fixate point *B*, the bottom of the arrow, the eye must rotate upward as in Fig. 3. The heavy lines in Figs. 2 and 3 represent that amount of light, leaving the top of the arrow, which can both traverse the lens system and enter the eye through the center of the pupil. The maximum extent of rotation within the visual field, with head stationary, was 31 for monocular and 34.5 for binocular vision. Hence any point lying within this range may be fixated by adequate eye movements. The fixational movements from a given point to any other point in this inverted visual field are always in the inverse direction of those involving the same points during normal vision. It is further evident that any movement of the head upward would cause the bottom of the arrow in Figs. 2 and 3 to disappear from the visual field and vice versa. This is necessarily true because the tubes of the apparatus containing the lenses are held stationary on the head by means of a head rest. Hence, fixational head movements remain the same as in normal vision. This is in direct opposition to Carr's statement that during lenticular inversion "ocular habits remain the same, while antagonistic head movements are required for fixation" (**7**, p. 38). Carr's position on this matter is hard to explain. He probably assumes that clear vision in the inverted binocular field of 34.5 is limited to the very center of this field. That this is not the case will be shown later in this paper. However, even if such an assumption were granted, we would still be at a loss to explain why Carr regards the fixational head movements to be reversed.

III. Experimental

It is generally recognized that fixational eye movements are necessary for reading and for visual pursuit of a moving object. Hence, a critical experiment for the determination of whether fixational eye movements occur during lenticular inversion would be one pertaining to reading, its rate and comprehension, and the clarity of perceived movement. Such an

experiment was conducted with six College Seniors serving as subjects.

The reading material consisted of digits and a sentence. The digits (387215964735128469735284) were printed upon two separate strips of cardboard, one of which was arranged for vertical and the other for horizontal reading. The digits which were presented vertically were printed in the reverse order from those presented horizontally. The sentence, which was presented horizontally, consisted of 2⅔ lines and read as follows: "The place of illusions in space perception is commonly misunderstood" (**7**, p. V). The lines of the two types of material measured 66 cm. All of the material was presented 1½ meters from the subject's eyes and thus subtended an angle of 24.5°. It should be noted that the average fixational movement, during horizontal reading of printed words, subtends a visual angle of from 1° 2′, according to Shen (**34**), to 4° 13′, according to Erdmann and Dodge (**16**). The average visual angle subtended during vertical reading is approximately the same. In view of the fact that the material employed in this experiment subtended an angle of 24.5° the number of fixations required to read one line of the sentence would range from about 6 to 20. Thus if this material can be read and comprehended in approximately the normal rate, in the absence of head movements, we are safe in concluding that eye movements must occur.

After the inversion apparatus had been carefully adjusted to the subject's head, his head was placed in a head-rest. A cloth screen hid the reading material from the subject's view during preliminary preparations for the experiment. The material was presented in the following order: the sentence, digits presented horizontally, digits presented vertically. For presentation the material was rotated 180° as viewed by normal vision, *i.e.*, stood on its head and reversed right to left. For reading the sentence, the following instructions were given: "In back of the screen is a sentence which you are to read. It will be presented for only four seconds, after which you are to repeat it aloud." Preliminary experiments with other subjects had shown that the sentence could be repeated correctly

after from three to five seconds exposure for normal vision. For the digits the following instructions were given: "In back of the screen is a list of digits. When I raise the screen you are to read them aloud as rapidly as you can."

Results—Three subjects reproduced the sentence correctly. One subject repeated it as follows: "The place of illusions in space perception is 'often' misunderstood," thus substituting the word 'often' for 'commonly.' Another subject omitted the word 'commonly,' and the sixth subject omitted the words, 'in space perception.' The digits were read in from 6.5 to 10 seconds, with a mean of 8.5 seconds. Horizontal reading was slightly more rapid than vertical reading.

It is thus seen that the material can be read during lenticular inversion, with the head stationary, in approximately the same time as during normal vision. This indicates that eye movements, irrespective of head movements, occur during inverted vision.

In order to test the clarity of perception during visual pursuit of moving objects, the experimenter moved the following objects across the subject's visual field, at the rate of about two seconds per object: watch, door-key, ink-bottle, small box of matches, yard-stick. The subjects were then asked to name these objects. No errors were made. The subjects also reported that these objects could be seen distinctly and without blurring as long as they remained in the visual field. It is obvious that in the absence of eye movements, with head stationary, this would have been impossible. If the co-ordinated compensatory eye movements were obviated by the use of lenses, the visual field would be blurred when there were any forms of head movements during lenticular inversion. This, according to all experimental findings, is obviously not the case. Although photographic recording of eye movements during lenticular inversion might be regarded as the final test for the presence of such movements, in the absence of head movements, the above experiments may be regarded as reasonably conclusive. Photographic recordings would, however, throw valuable light upon the latency of the various types of

eye movements and thus indicate the degree of interference or facilitation induced by the inverting lens system.

IV. Summary

1. Experiments on inverted vision, binaural transposition of sound, chemically and physico-chemically induced antagonism and facilitation of vestibular reflexes and pharmaceutical arousal of sensory and organic hallucinations have been described as effective methods for investigating space localization. The disorientating effects resulting from temporary abnormalities in the intra-sensory and inter-sensory-motor behavior of normal human subjects, and the recovery brought about through adaptation or learning, throw valuable light upon the growth, organization and operational characteristics of spatial habits.

2. A critical survey of the literature on distorted vision shows considerable diversity in the interpretation of experimental results. This difference of opinion may, however, be explained on the basis that authors have often confused at least two distinct types of distortion effects. The first is that investigated by Wundt (46) and subsequently by Gibson (18), dealing with adaptation to prismatically induced metamorphopsia. This type I have termed intra-sensory because its effects are primarily limited to the visual sense department. The second type, which I have termed inter-sensory-motor, was first investigated by Stratton (35, 36, 37) and subsequently by Wooster (45), Ewert (14), Brown (6), and a number of foreign investigators (3, 4, 21, 40). Inter-sensory-motor inversion effects are occasioned through active localization, and result from antagonistic inter-sensory guidance, from the antagonism between eye-movements of ocular and vestibular origin and from antagonistic conceptual habits.

3. The problem of upright vision involves active inter-sensory-motor localization and hence belongs to the second type of interference and adaptation. It should not be confused with the intra-sensory effects which involve merely the stability of the visual pattern. A confusion of the two types has led to the belief that continuous practice would ultimately

result in complete adaptation so that objects in the visual field would again be seen as upright with reference to body. However, it has been shown that the "feeling of normalcy" in the visual field following a period of continuous inversion, is merely an illusion resulting from a lapse of attention to the experience of inversion. The illusion is further attributable to the fact that the pattern of a seen object is not altered by the inverting lenses.

4. Some critics of the eye movement theory of space perception have held that experiments on inverted vision have neither supported nor invalidated the theory, because eye movements were either unaffected by inversion (**26**, **7**) or they were unnecessary for fixation or pursuit (**7**). However, I have shown that inversion induces antagonism between eye movements of ocular and vestibular origin and between eye movements and bodily movements guided by inter-sensory and conceptual processes. I have also shown that lenticular inversion does not obviate eye movements. In a following paper I will discuss the rôle of eye movements in the adaptive process during prolonged visual distortion.

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THE NATURE OF THE CONDITIONED RESPONSE: II. ALTERNATIVES TO STIMULUS–SUBSTITUTION

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Conventional conditioning theory subsumes an enormous range of facts varying from the development of a mechanical skill to the entrenchment of habitual attitudes and prejudices under the single law of contiguous stimulation. Association theory's three primary laws of similarity, contrast and contiguity were being reduced to the single principle of contiguity before the conditioned reflex usurped association's place (**34**, 289). For conditioning theory to assert the fundamental nature of contiguity was but to adopt a position already popular; to assert that the association was between stimuli and movements (rather than between ideas) fitted the temper of the times. It is not strange that the conditioning principle met with wide acceptance.

An examination of the substitute-stimulus principle (the most familiar form of the contiguity law as applied to conditioning) showed it to be inadequate to account for the dynamics of the conditioning process (**10**). It fails to suggest the descriptive differences between conditioned and unconditioned responses; it fails to deal adequately with the temporal order of events; it can not satisfactorily envisage the facts of experimental extinction. In spite of these failures many writers are reluctant to sacrifice a formula which has proved itself convenient. Some of these, instead of enforcing the formula by fiat, patch it with supplementary concepts to remedy its defects. Others who find the failures more damaging reject the principle entirely and account for the facts in other ways.

Amendments to Simple Stimulus-Substitution

The sterility of the pure atomism of substituted stimuli and reflexes gives rise to remedies for the contiguity principle which are reminiscent of the supplementation of association's primary laws with secondary ones. Analogous to Thomas Brown's secondary law of liveliness is the principle of dominance in conditioning; related to his emphases on diversities of state and prior habits of life and thought are the organizing principles of set or attitude now being introduced into conditioned reflex theory. It is to the credit of the associationists that they sensed the inadequacy of an oversimplified mental mechanics and proceeded to modify their views a century ago; the early statements of conditioned reflex enthusiasts were a return to a more naive associationism, and conditioning theory is now gradually retracing association's evolutionary steps.[1]

The concept of dominance has been introduced to meet the problem of direction of conditioning. If a child is given castor oil in orange juice, which will be the response conditioned? Will the child learn to accept the castor oil or to reject the orange juice? Razran (**21**) and Rexroad (**23**) argue that the more dominant of the responses will be conditioned. The term 'dominance' derives from the physiological work of Ukhtomsky, but its connotation has been broadened in its application to conditioning. There is always a danger lest a concept like dominance be used in the circular fashion common to the older explanatory uses of instinct. The pigeon returns home because of the homing instinct; we know it has a homing instinct because it returns home. A response gets conditioned because it is dominant; we know it is dominant because it gets conditioned.

Relevant facts may be found in the conditioning experiments of Stephens (**27, 28**) and Bernstein (**2**). Two stimuli are capable of eliciting similar responses of unequal magnitude. Let us call the stimulus which elicits the lesser response weaker, that which elicits the greater response stronger. If the stimuli are presented in sequence, the weaker preceding

[1] It would be grossly unfair to apply the term 'atomistic' to association in the form advocated by Carr (**3**) and Robinson (**24**) today.

the stronger, a conditioned response develops to the weaker stimulus; if the stronger precedes the weaker, the conditioned response develops to the stronger stimulus. When the stimuli are presented simultaneously, so that the responses coincide, practically no conditioning occurs to either stimulus (**2**, **11**). In other words, the order of succession determines conditioning, not the relative potency of the stimuli. Some principle of temporal organization is necessary; dominance alone is insufficient, unless, with Razran, time order is accepted as a condition of dominance.

Guthrie finds the principle of dominance superfluous since what occurs is what gets conditioned (**8**). He requires, however, some organizing principle, for what gets conditioned may be something which has not occurred before or it may be only a part of what happens when both stimuli are presented in sequence. His failure to consider time order as a significant factor is perhaps due to his acceptance of simultaneity rather than contiguity as the basic relationship of events in conditioning. Coincidence is non-directional and makes no provision for sequence; contiguity refers to adjacent or touching events, and allows for time order. The necessity for distinguishing between coincidence and immediate succession can be demonstrated by an example. If the end domino of a row of upright dominoes is tipped over, it reacts upon the next domino by actual contact, and initiates a series of events; the neighboring events are contiguous, but not simultaneous. A direction is implied; if the domino at the other end had been pushed over first, the sequence would have been different. Those who insist that simultaneity of cue and response is the basis for conditioning fail to make this essential concession to the experimental evidence that conditioned events are organized in time.

That the events in a conditioning experiment are an interdependent whole is well illustrated by experimental extinction, and it is in connection with these phenomena that Guthrie found it useful to introduce the notion of set or attitude (**8**). The similarity between conditioned responses and voluntary reactions makes it plausible to expect them to be operating

under similar widespread controls for which the term 'set' is convenient (**9**). Razran has recently offered a theory of conditioning types in man, based upon attitude (**22**). Others have noted the verbal nature of conditioned responses in man (**4, 26**). These supplementations do more than call attention to the complexity of stimulus-response relationships in conditioning experiments; they introduce terms such as direction, organization, means-end relationships, which are foreign to the logic of stimulus-substitution. To confess that the bald statement of association by contiguity is unsatisfactory because we oversimplify the items which are contiguous is one thing; to supplement the concept with heterogeneous organizing principles not coherent with it is another. The danger of doing the latter stalks all the attempts to amend stimulus-substitution into a satisfactory account of conditioning.

Teleological Explanation

Those writers who frankly favor teleology, believing it to be more appropriate to a science of behavior than mechanistic-atomistic explanations, may treat conditioning in either of two ways. They may relegate the facts of conditioning to an exceedingly minor place, as though the conditioned reflex is an interesting but unimportant curiosity revealed under special circumstances in the physiological laboratory, or they may reinterpret the facts of conditioning in teleological terms consonant with their systematic treatment of other types of behavior. The former practice has been the more prevalent. It is less cantankerous; it is exactly what we have done with other behavioral concepts such as tropisms. Few psychologists bother to reinterpret tropisms in spite of their vigorous championship by scientists who continue to write chapters for our handbooks (**5**). The tropism is admitted for insects and perhaps for young mammals on inclined planes, but we go ahead with our theories of behavior with entire disregard of them. So with conditioned reflexes: they are all right for physiologists working with dogs and other animals, and for those psychologists who insist on imitating physiologists by studying minute segments of behavior, but their limited scope

and essential artificiality exclude their importance for theory. The alternative, a serious attempt to study the facts from a teleological point of view, is less common. Tolman (**30**, **31**) has been careful to clarify the difference between his theory of sign-learning and the conventional theory of stimulus-substitution. His fundamental position is that we understand what happens in a conditioning situation only if we organize the events around the goal-seeking behavior of the organism. This procedure the orthodox conditioning theory does not allow.

Because conditioned reflexes originated in a materialistic-mechanistic tradition, and have been used as a support for the machine-like theories of behaviorism,[2] it is surprising to find that teleological explanations fit very neatly many of the facts of conditioning.

The problem of similarity or equivalence of stimuli and of responses is one of the most embarrassing for atomistic theories; there must be recurrences if the theories are to have meaning, yet repetitions of situations result neither in identical cues nor in identical responses. This difficulty is solved easily by teleology: stimuli are equivalent if their significance to the organism is alike; responses are equivalent if they accomplish the same end for the animal. Stimuli act simply as sign-posts directing behavior which satisfies needs. This statement is either axiomatic, and therefore simple, or it is very complex indeed. If one tries to analyze out the sequences of movements, secretions, and the like, which implement this sign-following and goal-seeking behavior, the simplicity disappears. One has only to study the elaborate constructions required by Hull to deduce the place of directing ideas (**12**), or to work out the principle of alternative routes (**13**) to be made aware of the complexity of the problem.

If one accepts the tendency of animals to follow signs in the pursuit of goals which satisfy needs, the facts of conditioning are readily explained. Some representative applications of teleology to conditioning follow:

1. The anticipatory nature of the conditioned response,

[2] This tendency is reflected in the desire to construct mechanical models of conditioning. For a recent popular summary of these attempts, see (**7**).

and its dissimilarity in other respects from the unconditioned response are understandable. The conditioned stimulus is a cue to the unconditioned stimulus and to the reactions which it evokes; because of this relationship, the conditioned response is either one of avoidance or one of preparation. The situation will determine whether or not the conditioned response resembles the unconditioned one.

2. Experimental extinction occurs when the expected goal fails to materialize. The probabilities in the situation will determine whether or not extinction is gradual or rapid. The more often the cue has been given without reinforcement by the unconditioned stimulus, the less probable on subsequent trials that the unconditioned stimulus will occur and the more feeble the conditioned response in anticipation of it. This analysis on the basis of probabilities assumes the hypothetical nature of behavior as discussed by Tolman and Brunswik (33).

3. Spontaneous recovery on return to the laboratory following an earlier extinction period may be attributed to the likelihood that the signalling value of the conditioned stimulus has been reinstated, a probability not infrequently confirmed. The tendency to begin looking for the next mail delivery after examining an empty mail-box provides an analogy. Having just looked and found the box empty, one is not tempted to look again immediately; lapse of time increases the probability that the box is again really a mail-receptacle and not a false cue.

4. Initial generalization and subsequent differentiation (discrimination) depend upon the selection of good from bad signs. Repetition is required in order to test conflicting hypotheses by the actual probabilities of the situation as it is set up by the experimenter.[3]

[3] In some of my experiments on conditioned discrimination (not yet published) the course of discrimination does not follow the subjects' verbal statements of 'insight' into which is the positive and which the negative stimulus. While from the experimenter's point of view the one stimulus will always be positive, the other negative, from the point of view of the subject the negative stimulus may at any time become positive (*i.e.*, be followed by the unconditioned stimulus). If reaction is according to probability, it is understandable why the discrimination develops slowly, although from the first few stimuli the subject recognizes that one stimulus is regularly reinforced, the other not. This interpretation was suggested to me conversationally by Dr. Jack Buel.

It would not be difficult to treat other facts, such as the development of delayed and trace conditioned responses, irradiation, disinhibition, from the point of view of teleology. The only possible experimental disproof of the teleological position would be to find conditioning under a situation so bizarre and stupid that the teleologist had to admit there was neither rhyme nor reason to it. Such a situation is not easily found, for the demonstrated circumstances favoring or hindering conditioning are intelligible from a purposive standpoint. Thus conditioning at intervals too short for the first stimulus to be discriminated as a cue for the second occurs with great difficulty; relatively non-adaptive responses such as the knee-jerk are very resistant to conditioning, unless they are made part of a larger pattern of responses (**25**); such non-voluntary responses as the pupillary reflex or the galvanic skin response are of long latency and they may be secondary accompaniments of anticipation. If one objects to teleology it must be on logical grounds; an effective protest can not be made on the basis of inapplicability.

The following statements may be agreed upon as a basis for discussing the relationship of teleology to conditioning:

1. *The findings of conditioned response experiments may be described in terms of teleology without doing violence to the facts.* Teleology has this advantage over simple stimulus-substitution.

2. *Teleological explanation of conditioning may be made consistent with its premises.* An explicit teleological formulation is logically preferable to an atomistic theory amended with heterogeneous dynamic concepts.

3. *Satisfactory teleological explanation does not preclude the possibility of other equally satisfactory explanations.* Systematic explanations may take different points of departure. The pupillary response to light serves a definite purpose in the normal eye and may be explained in terms of its purpose (*e.g.*, to regulate the amount of light falling upon the retina). It obeys, on the other hand, the general principles of reflex action and may be so explained.

4. *The scientific value of teleological explanation in comparison with alternatives must be judged by its predictive possibilities.*

There are two requisites for satisfactory scientific hypotheses: (1) quantitative formulation, so that predictions may be tested specifically, and (2) vulnerability, so that the hypotheses can not be allowed to stand if the predictions fail.

The teleologist has a passion for completing his system, and a finished system at the present stage of psychological knowledge is unlikely to meet either of the tests of good hypotheses. In the first place, we lack the interchangeable constants necessary for generalized formulas by which predictions can be made. The analogy between general psychological theory and general theories of modern physics breaks down here, because even the most highly generalized statements of physical theory always mesh with empirical data.[4] In the second place, even when approximate predictions are possible, teleologists often refuse to give ground if their predictions fail. For the teleologist, behavior *is* purposive, and the task of science is essentially to discover how particular behavior may be stated in purposive terms. If prediction fails, it is no disproof of purposiveness; the purposes operating were simply misunderstood. This is illustrated in Wheeler and Perkins' insistence that there is no trial-and-error (**35**, 358); the fact that fumbling behavior occurs can not contradict the insightful character of all behavior. It is possible in this manner to enforce teleology; it is not possible to disprove it. A failure of prediction contradicts only subordinate bases of prediction, never the larger hypotheses. A problem may have been too difficult for the animal; had the number of cues been increased or the severity of the obstacles reduced, the solution would have been achieved with rapidity and insight.

It is not fair to level the charge of excusing failure solely against the teleologists. Mechanists are equally prone to offer *ad hoc* hypotheses to account for the failure of their schematic

[4] Humphrey's conception of conditioning as a progressive four-dimensional integration lacks scientific usefulness because it does not have the physicist's tie-in with data. To say that organisms go about achieving four-dimensional integrations is to say only that organisms live in space and time. This is irrefutable, but it leaves the detailed problems of adaptive difficulty unclarified.

These excursions into system-theory do not detract from Humphrey's detailed and illuminating account of conditioning (**15**).

predictions of behavior. In view of the present state of prediction in psychology, neither the mechanists nor the teleologists are ready for the crucial series of experiments which will test their relative strengths. Both have too many alibis.

Debates carried on over the data of conditioning reflect the larger issues of scientific logic and system-making. Many of the discussions do not go on in an atmosphere of empirical data at all; when they do, the facts may be secondary to the logical issues of the controversy. The difficulties which beset the attempt to produce a decisive experiment are illustrated in a report by Tolman (30) answered by Miller (20). Both call upon evidence from experiments set up in reference to hypotheses. Tolman clings to his sign-gestalt position, although his evidence does not favor his own predictions. The evasion is that his rats had been over-trained before he introduced the shock which was to produce the crucial modification. Miller's experiments satisfied the predictions which he deduced from Hull's conditioning principles. It happens, however, that the success of his predictions depended upon the degree of perceptible difference between two goal-situations, the very differences which Tolman would expect to provide discriminable cues for a sign-learning animal. In this instance Miller performed a more satisfactory experiment than Tolman for the purposes of their controversy, but their theories are equally applicable.[5] Only when the systems have been more fully worked out, with the necessary mathematical constants provided, can there be a real test made on the basis of accuracy of prediction, the self-consistency of concepts, and the number of postulates needed. The time for such a test of systems seems remote.

The Physiology of Adjustment

When confronted with the detailed findings of the laboratory, our clearest formulated theories of conditioning are

[5] The systematist who is also an experimenter falls back on the statement that whatever else you think of his system, you have to respect it for the experiments it suggests. One must be careful to distinguish between the fertility of a system and the cleverness of those who happen to believe in the system.

found to be unsatisfactory. The simple stimulus-substitution formula does not cover all the relevant facts; teleological explanation, without doing violence to the facts, lacks precision in prediction. What have the laboratory workers themselves to offer, as they attempt to present their data systematically? One finds them using any terms which prove convenient for the handling of their data; they speak a jargon of neurophysiology, stimulus-substitution, and purposivism. Physiologists are especially prone to turn to physics and chemistry for the explanation of energy changes within the various bodily systems, and to propose functional explanations for the interrelationships between organs and between the organism and the environment. The tendency for identification with chemistry and physics is mechanistic; the tendency to explain by adaptivity is teleological. Because laboratory physiologists such as Pavlov and Liddell represent this point of view, it may be characterized as the physiology of adjustment.

Pavlov, although committed to a mechanistic position, readily fell into the use of teleological concepts as a convenience in the organization of his data. Expressions such as 'freedom reflex' and 'investigatory reflex,' commonly looked upon as unfortunate regressions from objectivity, were, after all, courageous attempts to describe clearly and succinctly families of behavior-acts which he saw in experiments. If one in reading his descriptions abstracts from preconceived notions of reflexes and disregards hypothetical physiological processes in the cortex, one finds Pavlov patiently describing what his critics discovered much later: differences in conditioned and unconditioned responses, anticipatory reactions, dependence of the success of conditioning on the particular animal and on its condition of health and motivation. Surely one can not accuse Pavlov of having accepted a naive stimulus-substitution theory. Pavlov's own formulations can be understood more clearly in teleological terms: the 'signalling' action of stimuli, an expression he used over and over again, anticipated Tolman's sign learning. The restricted freedom of the responses which were subjected to measurement in Pavlov's experiments

limited the contradictions to be expected between his results and substitution theories. His dogs salivated more or they salivated less; that is all that was consistently recorded. In experiments allowing more freedom, and those in which more samples of the total reaction are recorded, more discrepancies appear between conditioned and unconditioned behavior. When these discrepancies and variabilities become pronounced, experimenters tend to fall upon adaptiveness as a basis for explanation.

Liddell, James and Anderson (17) in a carefully elaborated series of experiments on pigs, dogs, sheep, goats and rabbits studied from the point of view of comparative physiology seem to reject entirely the stimulus-substitution notion in favor of an account in terms of adaptive response. They show that the conditioning procedure is a natural development of standard physiological practices; they are not introducing a new scientific logic into physiology.[6] The following quotation illustrates their freedom from the restrictions of a mechanistic description:

"*The pattern which the conditioned reflex exhibits depends upon the circumstances under which it is elicited.* It is not stereotpyed, but is, on the contrary, notably plastic. . . .

"We may think of the conditioned motor reflex in anticipation of the electric shock as an addition to the animal's defensive equipment. To be adequate, the reflex must fulfill the purpose for which it was established. That purpose is the protection of the skin against noxious stimulation. When warned of an approaching electric shock the 'conditioned' animal seeks to defend itself in the manner *appropriate to the situation.*"[7]

Liddell and his co-workers would not admit departing from the path of strictly objective physiology in making these remarks about adaptive responses; their descriptions seem to them quite as objective, and certainly more accurate, than

[6] The point of novelty which they stress most is the fact of individual differences and the importance of knowing the history of the animal. This is of course a commonplace observation in comparative psychology and reflects a change in the statistical outlook of physiologists rather than a change in scientific logic.

[7] Italics in the original (17, 54 f.).

the statements often made 'by psychologists' that the conditioned response is like the unconditioned response (16, 279).

Schlosberg's (26) experiments in conditioning the white rat included observations of behavior quite other than that evoked strictly by the unconditioned stimulus. He found many things happening and recorded faithfully all that he observed. His conclusions are that conditioning principles, as such, apply to very restricted aspects of behavior. While he warns against too abstract an interpretation of his findings and pointedly renounces attempts to explain results in animal behavior by the use of 'concealed indirect introspection,' his findings are intelligible only in an adaptive setting.

Culler and his associates (6), who are making excellent use of the conditioning process as a technique for studying discriminatory behavior, find it quite feasible to consider conditioned responses as adaptive, especially late in conditioning. If avoidance of shock is the reasonable outcome of the conditioned behavior, the animal calmly lifts his foot at the signal that the shock is coming, no matter how violent the withdrawal to shock may have been at the start.

The tendency to introduce notions like set and attitude to account for certain aspects of conditioning by those holding to the stimulus-substitution principle has already been mentioned. It should be pointed out that this usage harmonizes with the physiology of adaptation. As a further example, Rexroad's treatment of extinction may be cited. Extinction occurs because the unreinforced conditioned stimulus is not followed by a dominant response. It is not dominant for three reasons, the final two of which are clearly matters of adaptation:

"It [the conditioned stimulus] can not produce dominant salivation for three reasons: (1) the effectiveness of the conditioned stimulus is subject to counteractions or inhibitions by other stimuli, (2) the conditioned stimulus does not satisfy or terminate the drive which was involved in its development, and (3) the conditioned response is to some extent an anticipatory or readiness response, a type of response which can not be maintained if the external stimulations which are anticipated fail to appear" (23, 461).

The illustrations of adaptivity have been deliberately re-

stricted to experimental conditioned responses as previously defined (10). Whether conditioning principles are appropriate for analyzing behavior in alley mazes and discrimination boxes is a matter for separate consideration.

Adaptive physiology is for the most part a descriptive convenience. Its important characteristics are (1) an emphasis upon the context in which behavior occurs, and (2) a freedom from the restraint of abstract taboos against the language of teleology. If behavior can be shown to be part of a larger unit, such as locomotion, many more variations are coherent with it than if the behavior is conceived as a local reflex. Behavior which is part of a syndrome of escape is harmonious if it consists in appropriate struggling movements, no matter how complex. Insofar as it adopts larger contexts, adaptive physiology leans towards organismic or field theories, which need be no more teleological than the field theories of modern physics.[8] On the other hand, the cataloguing of behavior in terms of the ends served tends to imply teleological explanation. If the ends served are merely rubrics for the classification of data, there need be no departure from mechanistic logic; if the ends become explanatory of behavior, mechanism yields to teleology.

There is a difference between discovering that adaptive descriptions are conveniently applicable to conditioning and resting satisfied with them as final scientific formulations. That we eat to live is indisputable, but this is scarcely a scientific summary of alimentation. For the mechanist the fact of adaptation is not a solution; it merely poses a problem respecting the order of events.[9] The physiologists them-

[8] Claude Bernard as early as 1865 formulated the necessity for considering larger contexts in physiology (1). The system-theory advocated by Humphrey (15) is modelled on the concept of physico-chemical equilibrium, and is not as explicitly purposive as Tolman's sign-gestalt theory.

[9] This challenge has been accepted by Hull (14), whose writings represent a serious attempt to derive adaptive behavior from the postulated facts of conditioning which are expressed in terms of experimentally measured stimulus and response sequences rather than in terms of adjustment. The teleologist may point out that adaptive relations already exist in the basic conditioning data from which Hull's postulates arise, and that he does not therefore derive adaptivity *de novo*. He has, however, avoided teleological concepts in extending the principles of conditioning to purpose, goal-behavior, and the like.

selves, concerned with the detailed descriptions of experiments, are seldom bothered about the logical difficulties encountered in being at once mechanistic and teleological. This applies as well to many if not to most of the psychologists experimenting on conditioned responses while they remain in the role of experimenters.

Scientific Explanation and the Data of Conditioning

Scientific explanation consists in relating the facts of experiments to other data already familiar and accepted. The search for relationships continues until the scientist's curiosity is satisfied. The unlike phenomena of luminosity and temperature of a red-hot poker may be 'explained' by relating them to molecular vibrations. This explanation will satisfy any scientist for whom molecular vibrations are already a part of his stock in trade. Both stimulus-substitution and teleology are explanatory by reducing novel modes of behavior to more familiar ones. The substitution explanation of conditioning relates acquired performances to familiar and accepted stimulus-response sequences (*i.e.*, unconditioned responses).[10] The teleological explanation of conditioning reduces the occurrences within conditioning experiments to familiar and accepted goal-seeking behavior. Since goal-seeking behavior is taken for granted by the teleologist, his curiosity is not further aroused. Objections have been offered to both of these explanatory systems: to stimulus-substitution because its mechanical simplicity is illusory, to teleology because it lacks predictive precision.

Insofar as adaptive physiology attempts explanation at all it is, as previously stated, an unrefined mixture of mechanism and teleology, with the disadvantages of both. Its advantage lies in its freedom to describe what happens in the language which best suits the situation. This follows from the greater interest of experimentalists in exploring the possibilities

[10] If stimulus-substitution really occurred, this would be a satisfactory explanation, without recourse to physiology. It is matter of taste (or of curiosity) whether the unconditioned response is taken for granted, or whether it is explained by neurophysiology.

within their experiments rather than in arriving immediately at self-consistent theoretical systems. Somewhere within this practice of bowing to the experimental data must lie the key to a successful theory of conditioning.

Experimenters, regardless of theoretical commitments, are reduced to common ground in the gathering of observations, and in the first stages of summarizing and elaborating data. They all describe as accurately as possible the experimental phenomena, correlating antecedents and consequents. The believer in contiguity must describe in detail the secondary conditions of his experiments in order to account for obtained data. The teleologist has to suit his predictions to the cognitive capacities and motivational states of the animal; these cognitive capacities and motivational states require empirical description. The procedure for determining whether or not a difference is statistically significant is alike for mechanist and teleologist.[11] Can not the fact of agreement upon experimental method by all concerned be used as a starting point for theory? To those who accept an operational point of view with regard to the concepts of science,[12] the answer is clear. By taking as a point of departure the relationships discovered in experiments (in the form of tables, graphs, scatter-plots and their mathematical summaries) it is possible to construct a series of statements regarding events in conditioning experiments. These statements will constitute the tentative 'laws' of conditioning. *They are at present the only possible 'laws' with a solid empirical basis.* A structure of conditioning theory can then be erected upon the scaffolding of these separate principles. This is a detailed task, to which a further discussion will be devoted.

[11] The theoretical bias makes a difference in determining what experiment the experimenter chooses to do and hence what differences he measures. Personal idiosyncrasies also play their parts; certainly all scientists with similar theoretical outlooks are not equally fertile in inventing experiments.

[12] That operationism is becoming a common meeting ground for diverse psychological outlooks is indicated in its endorsement by the sense-psychologists of Harvard, notably McGregor (19), backed explicitly by Boring in a foot-note, and Stevens (29); by McGeoch (18), who carries on the Chicago tradition in learning and memory; by Tolman (32) whose system has been considered something distinct. Liddell and his co-workers have likewise characterized their interpretations as operational (17).

Summary

The failure of the stimulus-substitution formula (the most familiar form of the contiguity law as applied to conditioning) leads to several alternatives: (1) amendments to the principle, corresponding to the secondary laws of association, (2) teleological explanation, and (3) the physiology of adjustment. The first of these alternatives is criticized on the logical grounds that it saves an atomistic-mechanistic explanation with a patch-work of heterogeneous dynamic concepts such as dominance, set and attitude. The second, or teleological explanation, while it is consonant with the facts of conditioning, tends to become too confident and invulnerable; failure of prediction does not modify the basic hypotheses, and to that extent the hypotheses are not scientifically valuable. The physiologist's free use of whatever descriptive or explanatory categories he finds appropriate to his experiments permits him to treat fairly the range of data found in his experiments, although the mixture of mechanistic and teleological concepts combines in adaptive physiology the disadvantages of the other explanatory systems. The practice of correlating discoverable antecedents with measurable consequents is necessary alike for the believer in stimulus-substitution, teleology, and the physiology of adjustment. The resulting relationships, expressed in mathematical statements giving a generalized account of graphs and correlation-plots, are the nearest approach we now have to 'laws' of conditioning with a sound empirical basis. The erection of conditioning theory must be guided by such established principles.

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PSYCHOPHYSIOLOGICAL SYSTEMS AND ISOMORPHIC RELATIONS

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This comment on the nature of psychophysiological hypotheses grows out of an interest in the concept of isomorphism, the tenet of Gestalt psychology that the pattern of experienced perception corresponds in some of its formal relations to the pattern of the neural excitation that gives rise to it (11, 60–67; 7, 61–67). This hypothesis had seemed to me insufficiently validated for the seriousness of the consideration that is given it. I suspected in it the existence of some unrecognized premise, some habit of thought so familiar that it escapes detection in common sense, and I set out to look for it. I found myself studying isomorphic physiological relations at the periphery, or, more often anisomorphic relations that had earlier been regarded as isomorphic when speculation had had very little fact of which it could take account. Thus I was brought to a consideration of psychophysiological hypotheses in general, regarding isomorphism as nothing more than a very special case. However, I found that I wanted to see how hypotheses of this sort are formed, what kind of validation can exist for them in advance of the discovery of direct empirical proof, and what happens historically to them as knowledge increases. Such is the origin of this paper. The outcome is an attempt to restate the problem of psychoneural isomorphism in physiological monistic terms, showing that the operational substitute for dualistic isomorphism (although no longer an isomorphism) is of such a nature as to explain the ready plausibility with which the original concept has been accepted.

First, however, we must sketch a picture of psychophysiology, one that will show the nature of its central problem and the general form of the solution. We must begin with a

comment on the sort of systems that appear in the scientific description of psychophysiological events.

Cause-and-Effect vs. Systems

It is the conventional view that psychophysiology seeks the description of causal chains of events that occur in connection with the action of the nervous system between an antecedent stimulus and a consequent response. While this conception often serves its purpose satisfactorily as an approximation, it involves a great many difficulties that prevent its employment for rigorous description. Let us see what some of the difficulties are.

(*a*) The initial limit, the *stimulus*, can not be fixed definitely, but must be chosen in accordance with the convenience of the problem. For instance, in vision the stimulus may be the proximal stimulus (7, 79 f.) of the excitatory pattern at the retina, or the light as it enters the eye, or the object that reflects the light to the eye.

(*b*) The terminal limit, the *response*, is also not fixed. It may be the movement of a muscle, or the consequent movement of a bodily member, or a subsequent mark on a kymograph, or a spoken word. If *E* writes down the word that *S* speaks, perhaps *E*'s movement is the 'response,' which thus depends upon the nervous systems of both *S* and *E*.

(*c*) Even when the initial and terminal limits have been fixed in a given case, the series, as we know it, is never actually continuous. There are all sorts of gaps likely to occur between stimulus and response, and our ignorance of the intermediate neural terms is so great that Skinner (15) has suggested that psychologists had better give up the nervous system and confine their attention to the end-terms of such series, where observation is more immediate and the temptation for insecure speculation is less.

(*d*) When consciousness is introduced into this series—and surely *psycho*physiology ought to have something to do with consciousness—it is separated from the rest by gaps, and unprofitable speculation arises as to whether consciousness

parallels some part of the causal chain (parallelism) or is a link in the chain (interactionism).

(*e*) The lateral limits of the causal chain are also indeterminate. Hardly ever can a receptor cell and an afferent nerve fiber be supposed to act alone. The retina acts as a whole, or at least in Hecht's 'unit areas.' A pure tone spreads its effect in the organ of Corti. It is doubtful if a cutaneous receptor can ever be separately stimulated. Nor can a sensory effect always be limited to a single sense. The analysis of an orchestral harmony is much easier when sight of the instruments supplements the sound.

All these difficulties arise because we are actually dealing with a *system* whose interrelationships resist description as a simple series of causes and their effects. Most events are systemic. Even so simple a relationship as that expressed by Newton's 'third law,' that action and reaction are equal and opposite, does not show clearly a distinction between cause and effect. However, the nature of a system is best seen in those complexes where interrelated variables can be understood without the possibility of analyzing them into dependent and independent variables associated in pairs. Fig. 1 shows

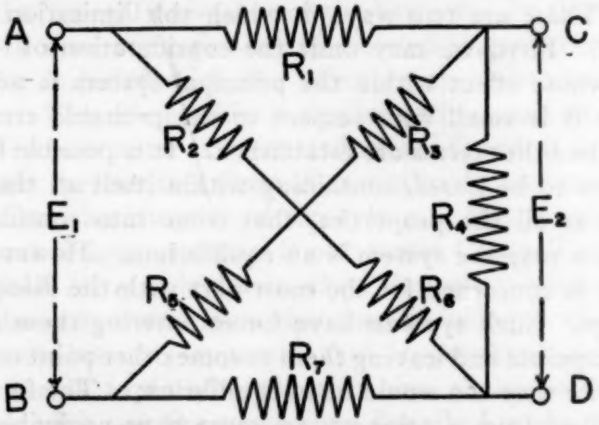

FIG. 1. An electrical system. The seven resistances, R_1, R_2, R_3, $\cdots$, and the input voltage, E_1, are known. When E_1 is kept constant across the network, the problem is to determine the currents through each of the seven branches, and how they change when any one R is altered. The output voltage, E_2, is any other potential difference in the net-work across which another system can be coupled. See text for conditions under which this net-work is (1) a closed system, (2) an open system, and (3) an incomplete system.

an example of an electrical system. The resistances, R, are constant, and we can imagine that a fixed input voltage, E_1, is maintained across the terminals A and B. The current through each of the seven branches can be calculated, but only by considering all the currents simultaneously. A change in the resistance of any one branch affects the current through every branch. No one of the branches is independent of the others, so that it can be considered first, with the others regarded as successive dependences. We have to set up seven equations expressing the interrelationships between the currents and their dependence upon the seven R's, and solve them simultaneously. This notion of a system as superseding the conception of cause-and-effect is quite fully established in modern science. It has been introduced into psychology as Gestalt. Excellent discussions of the nature of a physical system have recently been given by Humphrey (**6**, 7–39) and by L. J. Henderson (**5**, 10–19, 74–81).

Ideally there is only one system, the universe, including all time and all space (cf. **6**, 20 f.). Everything is related to everything else. Practically, however, it is necessary, for the solution of particular problems, to distinguish small finite systems. There are two ways in which this limitation can be effected. First, we may omit the consideration of 'lateral' terms whose effect within the principal system is negligible because it is small with respect to the probable error with which the other terms are established. It is possible for such a system to be *closed*, containing within itself all the forces, as well as all the properties, that come into consideration. In such a case the system is an equilibrium. However, psychology is concerned for the most part with the disequilibria of action. Such systems have forces entering them at some point or points and leaving them at some other point or points. Otherwise they too would be in equilibrium. The forces that are derived from a prior system must have negligible effects on the prior system; otherwise they would react upon the prior system, alter it and change its effect upon the principal system—which is to say that the two systems would be so related by interaction that they would have to be treated as

a single system. The same point applies where the principal system affects a posterior system. There must not be more than a negligible reaction of the posterior system upon the principal system if the two are to maintain their integrities as separate systems. In this manner we come upon a practical rule for the establishment of systemic boundaries: a disequilibrium must be an open system, accessible to influence by some external system and able to affect another external system, but these external relations must be asymmetrical if the system is to retain its isolation. Action between systems must be one-way; if it is mutual, the systems do not remain discrete. Let that statement stand for the definition of a system.

These distinctions can be made clearer by a further consideration of Fig. 1. E_1 is the input potential across the terminals A and B. We can regard E_2 as an output voltage across the terminals C and D. If E_1 is kept constant and no connections are made to C and D, we have a system, but an *open* one accessible to the system that supplies the voltage E_1. Then, if the potential across A and B is furnished by a battery, E_1 will vary when some R is changed, because E_1 will depend upon the internal resistance of the battery. In this case the net-work does not represent an isolated system; the battery must be included with the other seven resistances in the system. The relationship is similar with regard to the output. If a voltmeter is connected across C and D, it draws a negligible current and alters E_2 a negligible amount: it does not violate the integrity of the system. But, if some other apparatus of low resistance is put across C and D so that an appreciable current is drawn, then the new apparatus reacts upon the system and must therefore be included within it.

It now becomes apparent that in psychophysiology we have after all to do with causal chains, chains in which the links are such systems as we have described in the preceding paragraph. These systems are open disequilibria which transmit action in the general direction of 'stimulus' to 'response.' The complete action is a progression from system to system, with the reaction of any system upon its preceding system

regarded as negligible. 'Lateral' systems beside the principal train must be related to the train only in negligible ways. Negligibility is relative; what is negligible for one problem may not be for another. Thus at a sufficiently high molar level we find that we have after all the essentials of the cause-and-effect relation holding between open systems. This general pattern applies in the present instance because the nervous system is insulated laterally into pathways, and synapses or changes of media in receptors or effectors provide boundaries across these pathways where action is forward and reaction is negligible. In other words, the successive systems approximate each the net-work of Fig. 1, where E_1 is maintained by a preceding system and is independent of the value of any R in the network, and where E_2 is unaffected by any of the properties of a subsequent system. If these conditions in respect of E_1 and E_2 are not met, then the system must be enlarged until they are.

Correlation and Sufficient Reason

This conception of causal systemic series puts us in a position to understand why some effects seem to be founded upon sufficient reason and why an observed correlation seems in itself an insufficient reason for an effect.

Observed correlation is the basis of all scientific fact. The simplest law of the functional dependence of one variable upon another is nothing more than the observation of the concomitant variation of the two, usually by an experimental method where the independent variables (cause) and the dependent variables (effect) are distinguished. The notion of cause-and-effect in science is based upon the pairing of a dependent variable with an independent variable in experimental observation, which always requires the isolation of the variables from the continuity of nature. However, the method of correlation also extends to the establishment of systems. We have said of Fig. 1 that, if the input voltage and the resistances and the connections of the net-work are known, the seven simultaneous currents can be computed. The input and the seven resistances are eight simultaneous

conditions of the seven simultaneous currents: change any condition and you change all currents. However, the system involves no appeal to the notion of a serial cause-and-effect, for the currents are concomitant functions of all the properties of the system.

The objection to correlation is sometimes urged that it is discontinuous, that it represents action-at-a-distance. However, this supposed 'defect' is inherent in the nature of correlation, which obtains always between isolated variables. Nevertheless, these discretely correlated terms may be used to infer continuity, just as the plotted points of some functional relation come to be regarded as implying a smooth curve. In the same way a molar system is considered to be continuous spatially, temporally, and in respect of every other dimension of variation. (The discontinuities of electrons and quanta need not concern us here.) Thus one has, by an inference of continuity, very much more in a system than has ever been actually observed. In a statement of the properties of a system, one establishes, from a finite number of observations, dicta about an infinitude of cases. And these generalities remain established so long as new instances continue to satisfy them. It is this reification of discrete observations into systemic continuities which seems to provide a sufficient reason for the new facts which are subsumed under the properties of the system. Let us have an illustration.

The sun rises in the east. That is merely an observed correlation except as its two terms are intended to be generic. *That flaming orb always rises over that hill* is a correlation and thus a fact, but it seems senseless. There is no sufficient reason for it. But to say *The earth rotates toward the east in respect of the sun* is to describe a system and thus to provide a sufficient reason for the flaming orb's always rising over the eastern hill. In other words, a reason is sufficient if there is enough of it! A system is so much larger than a correlation that it will bring any observed relation, which can be subsumed under it, well above the threshold of credibility.

Psychophysiological Progression

We have said that psychophysiological events tend to progress through the organism in the afferent-efferent direction. This progression is represented by the successive excitation of a series of systems. The action gets from one system to a next because there is some term common to the two successive systems. If Fig. 1 represents an electrical system, then its input, E_1, must be the output of some preceding system; and its output, E_2, may, if the progression continues, be the input of a succeeding system.

Let us consider as an example present-day knowledge of the systems participating in auditory perception (*cf.* 4).

We may take as a first system the air. It is displaced initially by some vibrating body, perhaps a loud-speaker, which involves various mechanical systems that in turn depend upon electrical systems. It is because we must start somewhere that we begin arbitrarily with the air. If the sound is a continuous tone, produced in the presence of reflecting surfaces, standing waves are set up. The stimulus available at the drum-skin is therefore a function of the whole aerial system. It is not wise for us to try to divide this system into smaller ones.

The middle ear may be taken as a second system. The displacement of the drum-skin is a common term for the two successive systems. The action is essentially one-way, for the nature of the aerial vibrations is not altered more than a negligible amount by the properties of the middle ear. Thus we can put a boundary between the two systems. However, the middle ear has such properties that the wave-form at the drum-skin is distorted by the time it reaches the oval window.

The cochlea is the third system. Its mechanical action has to be regarded as a whole. Its presence does not radically affect the action of the middle ear (anterior system), and the presence of the receptor cells (posterior system) does not radically affect its action. It can therefore be distinguished as a separate open system between the oval window and the receptors.

The "cochlear response" is the name given to the elec-

trical phenomena found at the round window and other parts of the cochlea, a response observed by amplification of the resultant potentials (4, 970–976). It is supposed to represent events in the receptors cells, since it is known not to correspond with events in the nerve. It would seem, therefore, to be a fourth system.

Then comes the action of the VIIIth nerve, as it is now understood by the amplification of its action potentials. It is quite clear that it is a fifth system, separate from that of the "cochlear response," and that the one-way criterion is satisfied at the joint boundary.

There is some indication that the auditory tract at levels above the VIIIth nerve is divided into several subcortical systems (4, 980), but the data are meager.

The question arises as to what we are to do with *consciousness* in such a progression. In any monistic view, consciousness, if the term is used at all, becomes a word for some neural system or relationship between neural systems. However, the word generally connotes a dualism, and thus a difficulty arises. Enough has been said about the laws of consciousness, from the Associationists to Wundt and from Wundt to the Gestalt psychologists, for us to realize that consciousness is a system. Moreover, it is not a closed system. According to parallelism, its input is open. According to interactionism, both its input and output are open. Nevertheless, a dualism forbids a common term which joins the psychical system to the physiological, and consciousness is thus placed in a unique systemic position. There is nothing novel in the discovery of a uniqueness of consciousness under a dualism, but it is plain that this relationship forbids the possibility of there being a 'sufficient reason' for consciousness being as it is. Consciousness can be correlated with a neural system, but its action can not issue from the action of a neural system in the way that the action of one neural system issues from the action of another.

We must return to this problem of consciousness later when we discuss psychoneural isomorphism and the possibility of a sufficient reason for consciousness. Let us pass now to the meaning of isomorphism in general.

Isomorphic Relations

Isomorphism is a term invented by Köhler (11, 60–67) to stand for the theoretical notion that the spatial order of perceptual experience corresponds to the spatial order of the underlying neural processes. It signifies a particular hypothetical form of the unique psychoneural correlation. We shall examine this hypothesis later on, but at present we may stop to consider the nature of the conditions under which we should find the isomorphic relation between events in different systems.

The fundamental rule is very simple. In the case of the sort of open systems in series that we are discussing, *any event in one system is a joint function of the properties of that system and of the input which the system has from the preceding system.* Events between two successive systems can not very well be isomorphic in respect of any attribute unless the systems have similar properties. This rule would seem to make the isomorphic relation rare, and so special a case as not to be interesting as a topic of general discussion. However, there are three reasons why the principle can not be ignored.

(1) Isomorphism is the rule and not rare in the *propagation of an irreversible change through a constant medium.* The transmission of light in vacuo or the transmission of the nerve impulse along a single fiber are instances. In the latter case one is free to take any segment of the fiber as a single system, since it satisfies all the criteria of a separate system. Assuming that the membrane hypothesis of conduction holds, we find that the impulse in any segment of nerve fiber is isomorphic with the impulse in preceding segments and succeeding segments. In other words, the isomorphic transmission of an event is possible, and consists in the maintenance of the form of the event during its propagation. This special case fits the popular notion of causal chains which we have had, in general, to abandon in favor of systemic serial action.

(2) Because this simple kind of transmission is an hypothesis easy to imagine in the *psychophysiology of perception*, it has often been employed in the history of speculation about perception. The next section of this paper takes up this topic.

In it we must remember that the presumption always lies against isomorphic correspondence between serial events in successive systems. The burden of proof to show adequate reason for isomorphic relationship rests upon those who would apply this hypothesis to a given case, and the general rule is that events are not likely to be isomorphic between systems unless the properties of the systems are known to be similar.

(3) Finally there is the specific question of *psychoneural isomorphism*, which assumes importance because it has historical prestige and distinguished advocates. We shall consider this conception presently.

Isomorphism and Parsimony

The outstanding characteristic of perception is its correctness. Its chief function is to provide a correct picture of the external world. Illusion is the exception, for ordinarily the mind is adequate to reality. This fundamental truth invites the introduction of isomorphic relationships into perceptual theory. In what simpler way, the theorist asks, could perception be correct than by mirroring the external object? If his conception of mind is predominantly empiristic, then it is quite natural for him to think of perception as an isomorphic transmission of the properties of the perceived object.

Such indeed was the *image theory of perception* held by Epicurus and later by some of the scholastics. Epicurus' view was that fine images, exact replicas of the perceived bodies, are discharged from the surfaces of the bodies and reach the soul by way of the organs of sense. The isomorphic principle here involved is so natural that it has persisted in some form or other until the present, and most progress in the theory of perception has consisted in the overthrow of this principle in some special context. Locke's doctrine of primary qualities involved the principle, but his doctrine of secondary qualities gave notice that isomorphism between the object and perception can not be regarded as universal. It must have been Epicurus' view, embodied in common sense, that Johannes Müller was combating in his theory of the specific energy of nerves. Müller's theory was not new:

Thomas Young and Charles Bell had held it as a matter of course before him, and yet Müller had to polemize in favor of the theory and Helmholtz had later to proclaim its fundamental importance, all because the Epicurean view was so strongly established in common sense. What was it that Müller said? That we are directly aware, not of the objects of sense themselves, but of the states of the nerves that these objects affect. That does not seem a very difficult doctrine to get accepted; but it was difficult for Müller because he had to prove that the states of the nerves and the properties of their stimulating objects are anisomorphic, as he did by marshalling the evidence for 'inadequate' stimulation (*cf.* **1**, 76–94).

Recently Köhler has criticised what we have come to call the 'constancy hypothesis,' the assumption that perception and its stimulus have a constant relation, and that the form of the perception depends only upon the properties of the stimulus (**8**). This objection is the proper objection of Gestalt psychologists to isomorphism: the perception depends in part upon the laws of the system to which it immediately pertains and only in part upon the properties of the stimulus.

A special constancy hypothesis that has long dominated psychology is the view that the attributes of sensation—quality, intensity, extent and duration—are correlated one to one with dimensions of variation in the stimulus. For example, the pitch of a tone was supposed to be a function of stimulus frequency only; loudness was supposed to vary only with stimulus energy. Now we know that pitch and loudness are each joint functions of frequency and energy, and that other tonal attributes like volume and density are also joint functions of the same two stimulus variables (**16**). I have shown elsewhere—partly in penance for my own too ready acceptance of attributive constancy in the past—that theoretically the number of attributes of a sensation is entirely independent of the number of effective dimensions of variation in the stimulus, and that, as long as the attributes have different laws of functional dependence upon the stimulus, they will remain distinct (**3**). This relationship holds because the

attribute may be a function, not only indirectly of the properties of the stimulus system, but also of the properties of all those other systems in the progression that are antecedent to the establishment of the attribute.

Isomorphism is a parsimonious concept; it is a simple relation that economizes thought. When no contradicting facts are known, it is the natural assumption to make. "Entia non sunt multiplicanda praeter necessitatem." That is William of Occam's 'razor' and Sir William Hamilton's *law of parsimony*. However, there are in this case so many contradicting facts. The total system that includes the event called a perception involves many smaller, different, successively excited systems, and these systems impose their nature on the event. "Entia sunt multiplicanda propter necessitatem," we had better say. In view of the complexity of the media, simplicity of transmission is suspect (*cf.* 1, 487).

My point is that these isomorphic relationships are natural and easy to assume in the face of ignorance, but that they generally prove false when detailed knowledge of the systems involved begins to be available. To make this point quite clear, I beg the reader's patience while I give an historical illustration.

In 1675 Isaac Newton proposed what we should call now a theory of color vision (14). At that time Newton knew about the periodic nature of light and believed light to be vibratory, for he had not yet espoused the corpuscular theory. He suggested that the various rays of light would excite vibrations in the retinal terminations of the optic nerve, "the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bigness, strength, or power"; and he concluded: "these vibrations will run . . . through the optic nerves, into the sensorium; and there, I suppose, affect the sense with various colours, according to their bigness and mixture; and the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green, and a confusion of all with white—much after the manner that, in the sense of hearing, nature makes use of aerial vibrations of several bignesses to

generate sounds of divers tones, for the analogy of nature is to be observed." Newton supposed that there were seven colors, analogous with the seven musical notes of the octave. He had discovered the now classical laws of color mixture, including the then astonishing one that white may be a mixture of all the colors—presumably of all seven colors. What could have been more natural in the face of the ignorance of that day than to suppose that these frequencies are conducted by the optic nerve to the "sensorium?" So Newton assumed isomorphic transmission and put forward a theory that had at this point some resemblance to Epicurus' image theory of perception.

A century and a quarter later Thomas Young faced the same problem. There had not been much increase in relevant knowledge, but the many black absorption lines in the spectrum (later measured by and named for Fraunhofer) had been discovered, and it was known that there is an "infinitude" of kinds of light, not merely seven colors. Newton could suppose after the analogy of tones ("and the analogy of nature is to be observed") that seven colors could fall upon a single spot in the retina and their individual frequencies be maintained, so that the sensorium could perceive the specific "confusion" that is white. Young could make no such simple assumption for the infinitude of colors that compose white light, and he was thus obliged for a retinal theory of color to appeal to the properties of the retinal system.

Young wrote in 1801 (**19**): "As it is almost impossible to conceive of each sensitive point in the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited, for instance to the three principal colours, red, yellow, and blue, of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of these particles is capable of being put in motion less or more forcibly by undulations differing less or more from a perfect unison; for instance, the undulations of green light being nearly in the ratio 6½, will affect equally the particles in unison with yellow and blue, and

produce the same effect as light composed of those two species; and each sensitive filament of the nerve may consist of three portions, one for each principal colour."

Thus was born in its essentials the theory that is now called the Young-Helmholtz theory and which is the accepted principle today for hypotheses concerning color vision. Newton, believing in the existence of only a few colors, could suppose their isomorphic transmission at the retina. Young, knowing that there are very many colors, could make no such assumption. He had to look to the retinal system for properties that would so change the form of the event as to provide a 'sufficient reason' for the laws of color mixture.

Psychoneural Isomorphism

The notion that there is a similarity between perceptual relations and the underlying relations in the brain is so nearly implicit in the doctrine of psychophysical parallelism that it would seem that this hypothesis must be quite old. At any rate, G. E. Müller formulated five axioms concerning it in 1896 (**13**, 1–4). However, the principle has recently been given great importance by the Gestalt psychologists. Köhler laid down the rule in 1920 (**9**, 173–195, esp. 193) and coined the word isomorphism for the psychoneural correspondence of spatial orders in 1929 (**11**, 60–67). Koffka espouses the principle in general (**7**, 61–67). Wertheimer made use of it in discussing a physiological hypothesis for the phi-phenomenon (**18**, 246–252).

It should be plain from what has been said that the presumption lies against an isomorphism that represents a correlation between the two terms of a dualism. If consciousness and the brain are the terms of a metaphysical dualism, then we have two incomparable systems that would not be likely to have the identical properties that are implied by the isomorphic relation. If consciousness and the brain differ merely as the terms of an epistemological dualism, still one should not expect such different operations of knowing to imply identical properties. The presumption is strongly against isomorphism in a dualism, and, since *psychoneural isomorphism*

describes a dualistic relation, the presumption must lie against it.

However, a fact can easily annihilate a presumption. Let us therefore examine the factual situation in respect of psychoneural isomorphism. There are three cases to consider.

(1) There are the cases where the terms of both the phenomenal and the neural systems are alleged to have been observed and correlated. This is the sort of observation that would immediately rout a presumption to the contrary. The difficulty is that there are almost no such cases that are univocal. As far as I know there are none at all for intensity, duration or quality (what would quality be neurally?). There may be some for space, but they are very general, approximate and inferential. Koffka cites experiments that show that some principles of spatial organization in the brain resemble principles of spatial organization in perception (**7**, 61). Much of Lashley's work can be given similar implications (*cf.* **2**, 94–107). Perhaps it may be said that recent research tends more toward the support of the theory of the projection of the periphery upon the center, and thus perhaps, in view of the adequacy of perception, to imply an isomorphic relation between the projection field and the perception. On the other hand, temporal, qualitative and to some extent intensitive differences are also supposed to have spatial representation in the brain, so that it appears that this evidence can not be directed toward any simple isomorphism. This paper is not the place to review a large literature that is equivocal as to the point in question. Perhaps it is enough to say that the hypothesis of isomorphsim hardly needs to be considered in this case. It is certainly not yet proved. Much more exact neurological knowledge is needed. If it could be proved, the isomorphism would probably be forgotten in the presence of the observed correlations. The present stress on the hypothesis shows that it is still speculative.

(2) On the other hand, there are cases where no claim is made for a directly or indirectly observed neural term. In them, I think, the presumption against the isomorphistic hypothesis applies in all its force. Perhaps one such case is

the correlation of "experienced order in time" with corresponding temporal relations in the underlying physiological context. I think that this is the relation that Köhler has posited (**11**, 65), although he is so guarded in his sentences that I am not sure. In any case it is a common parallelistic view, and there is no observational evidence for the neural term. Such inference as is available makes it appear that the temporal factors ought to become spatialized in the brain before they are discriminated, as even Koffka indicates (**7**, 452). Operationism has a way of avoiding this difficulty of the neural spatialization of time, but the principle is not an isomorphism (or isochronism) and so does not form a proper digression here.

(3) A special class of cases is where the observed terms are all phenomenal but appear to be connected by physical laws. Wertheimer's assumption of a cortical short-circuit as underlying the phi-phenomena is a case in point (**18**, 246–252). Here it appears as if a final perception drains an initial one when the time between the two is not too short nor too long. (This argument is strengthened when Korte's laws are also known, but we shall not complicate it here.) However, the notion of drainage or short-circuiting is physical, and it may have appeared more plausible to Wertheimer to suppose that a relationship of a physical order within phenomenal experience must, since it is physical, imply an isomorphic basis in the brain.

It seems to me that Köhler's discussion of the physiology of the Weber function (**9**, 211–227) is a similar instance of this sort. The phenomena are such relationships as they would be if certain physical principles applied to them, and these principles, being physical, can be localized in the brain. Similar comment can be made on the mnemonic trace which Köhler (**10**, 137–148, 165–174), Koffka (**7**, 423–464) and others have considered as a neural entity, following physical laws of growth, decay and assimilation. Both these views imply an intensive isomorphism that is founded, not on the observation of neural states (*e.g.*, summed cortical action potentials), but on purely phenomenal data.

In these cases, I think, it is fair to argue that the presumption against isomorphism should be respected. If the data are phenomenal, let the explanatory concepts also be localized in the phenomenal world without the use of insecure isomorphic bridges across the dualistic gulf to the brain. A trace is a reasonable hypothesis. It is the residue of a no-longer-conscious sensation. It exists because it continues to have determinable functional capacities. Why force it into the brain when we have no physiological evidence for it, if we *as dualists* do not localize phenomena in the brain?

My general conclusion is that psychoneural isomorphism has not in general been validated, that as an *apriori* hypothesis it is too improbable to be safe, and that dualists should be warned against it. (Monists will not need it.)

A Restatement of Psychoneural Isomorphism in Physiological Terms

It is obvious that the difficulty which lies beneath the general assumption of psychoneural isomorphism is the impossibility of finding in physiology any sufficient reason for phenomenal consciousness. If consciousness is a system that is not identical with the underlying neural system, then there is no reason to suppose that events in the one system will mirror events in the other unless the two systems can be shown to have similar properties. But how can the two horns of a dualism be supposed to have similar properties, except as similar properties are separately and empirically established for each? What we need, if we are to assume psychoneural isomorphism, is (*a*) some knowledge of the properties of phenomenal experience, a knowledge which we are only beginning to have, (*b*) some knowledge of the properties of the underlying neural system, a knowledge which is also just beginning, (*c*) evidence that the properties of the two systems are similar, evidence which we have not got, and (*d*) at least a plausible theory as to how the two systems intercommunicate, whether by common terms or otherwise. If we could put all these neural and phenomenal events into two communicating systems, then we should have

the 'sufficient reason' for consciousness. However, I do not see how dualistic modes of thought can ever provide a plausible conception of psychoneural communication.

Nevertheless, I think we can have a sufficient reason for consciousness if we want it. In order to get it, we must first enquire concerning the nature of the 'immediate experience' that lies apart from the rest of scientific reality, that can be correlated (it is said) with physical reality but not systemically integrated with it. That enquiry is answered—and I do not see how else it can be answered—by the defining of *immediate experience* in terms of the operations by which it is known (**17**, 520–523). Since introspection is neural and behavioral, consciousness must be too. Consciousness is the capacity of the organism for specific differentiated response (discrimination), for an organism knows what, as an organism, it distinguishes, and what it can not distinguish does not exist psychically for it (*cf.* **2**, 221–236). Thus the problem of psychophysiology becomes the problem of discovering what differentiation in one system implies a specific differentiation in some antecedent system. With such conceptualization consciousness becomes, like every other event, a matter of systemic relationships. It is not so much that consciousness is integrated with neural systems as that it *is* certain integrations of neural systems (*cf.* **12**, 330–343).

On this view the problem of isomorphism dissolves in a knowledge of the functional relationships which obtain between variables of neural systems, and I urge that it requires some such view to strip isomorphism of the mystery of magic that has always clung to any bridge between the body and the soul (think of Descartes! think of Fechner!). Psychology will certainly face reality more securely if it can achieve this release from domination by the magic isolation of an insubstantial soul.

On the other hand, there is a monistic and systemic conception of the relation of introspection to the brain which is a physiological account of 'psychoneural' isomorphism. I offer it as the proper substitute for a concept that seems to

me to become less and less plausible and useful the more its meaning is examined.

Consciousness — immediate experience — introspection — they are all, I have argued, matters of discrimination. Discrimination can always be reduced to choices between *A* and *not-A*. In fact, in science discrimination usually is dichotomously limited. All readings of scales are choices between coincidence and not-coincidence, and other observations are most reliable if reduced to the discrimination between identity and difference. Though we may have more convenient gross descriptive methods, at least we can say that all science could be realized if the observer's discrimination were always directed upon the dilemna between *same* and *different*.

Thus introspection can be envisaged as action of the kind *A*-or-*not-A* in a terminal system. The determinant of *A*-or-*not-A* lies not in the properties of the terminal system that is directly observed but in an antecedent system. We know that specificity of reactions of this sort is not determined in peripheral efferent channels. The efferent neural paths may be quite different: *A* may be the movement of the right hand, and *not-A* the movement of the left hand. It is also possible by instruction to shift the differential response from the hands elsewhere, perhaps to the feet or to the vocimotor apparatus. Still the same discriminations remain. It thus appears that the crucial determiner of the specific response must lie in a system anterior to the peripheral efferent systems. The determination of the dichotomous discriminatory choice must be pre-efferent, because the alternative efferent paths are isolated from each other, and because the particular pathways used are irrelevant since a given discrimination can be switched onto various pairs of paths. It is plain that behavior at the periphery *qua* movement is not significant. It is significant only in respect of what it implies in the system whose differentiation determines the discriminatory behavior.

It is important to note at this point that, in a series of communicating open systems, the differentiation of some particular system, *Q*, may be determined by the differentiation

of some other system, K, antecedent to Q. For instance, the event k_1 in K may determine the event q_1 in Q, and k_2 may determine a q_2. There need be no similarity between K and Q, nor between the k's and the q's. There is merely a one-to-one correlation of a casual order between each k and its q. In such a relation the events in the system K *imply* the events in the system Q; or, we may say, the events in K *mean* the events in Q; or, in still other words, K *describes* Q.

In this sense it is true that *introspection implies, or means, or describes, the differentiation of some pre-efferent neural system.* The relation is not isomorphic because the events in the terminal system (introspection) are not like the events in the crucial antecedent (central neural) system. However, it is true that the meaning of the introspection—its meaning, not its content as motor events—does resemble the 'brain' pattern in the sense that it refers to it.

There are two ways of regarding this situation, and one seems to be isomorphic and the other not; yet there is really no essential difference between them. (1) We can say that, on the basis of introspective data, a set of psychic reals is set up. Such a real might be a sensory trace, which diminishes in degree with time. The trace is described by those dichotomous discriminations which are the immediate motor content of introspection. However, these discriminations are determined by and therefore imply (mean, describe) the crucial central determining system. Hence the neural system and the psychic reals are isomorphic. (2) On the other hand, it is apparent that we gain this psychoneural isomorphism by means of an unnecessary step. We do not have to posit the independent existence of the psychic reals at all. The argument that shows that the psychic reals are isomorphic with the determining neural system is an argument that makes introspection directly descriptive (implicative) of the neural system itself. Thus there seems to be nothing more to isomorphism than the tautology that, since introspection means (is determined by) an antecedent neural system, the meaning of introspection is that neural system.

Here, in the logic of this last paragraph, lies I think the explanation of why the hypothesis of psychoneural isomorphism retains importance in spite of insufficient evidence. There is no ground for it in the known systemic relations of two dualistic systems. However, the Gestalt psychologists, though speaking dualistically of phenomena and physiological processes, doubtless intuit the fact that introspection itself is a physiological event which reflects the differentiation of central processes, so that it becomes a description of the neural processes themselves (*cf.* **11**, 67). The paradox of this kind of isomorphism is that you can establish it only by annihilating it. To establish it you have to let dualism go and consider inter-communicating neural systems; and then you find that the isomorphism which you have is the implication by a terminal system (introspection) of the state of an antecedent system (brain). Psychoneural isomorphism reduces to nothing more than this: ***description implies its object!***

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